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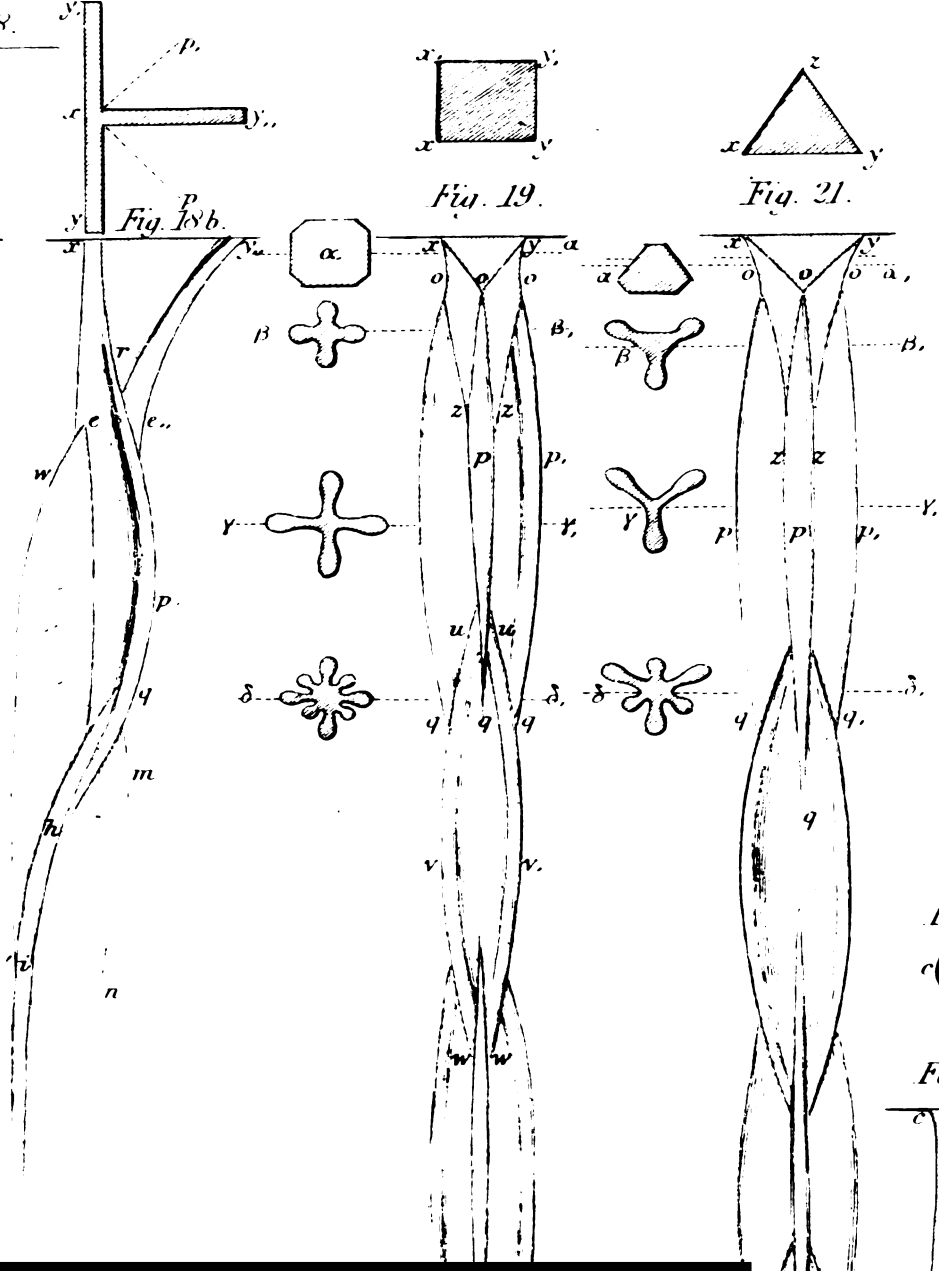

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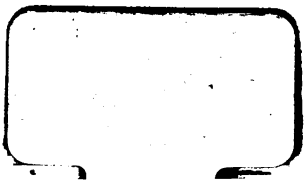

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

VOL. XI.—FOURTH SERIES.

JANUARY—JUNE, 1856.

LONDON.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;

SOLD BY LONGMAN, BROWN, GREEN, AND LONGMANS; SIMPKIN, MARSHALL AND CO.; WHITTAKER AND CO.; AND PIPER AND CO., LONDON: —BY ADAM AND CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH AND SON, GLASGOW; HODGES AND SMITH, DUBLIN; AND PUTNAM, NEW YORK.

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VII

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATA IN VOL. X.

Page 314, line 26, *for* displaced *read* injured.
— 316, — 13, *for* 108 *read* 168.
— 324, — 23, *for* sunk from 1 to 3 lines *read* sunk 1, at the most 3 lines.
— *ib.* — *ib. for* from 5 to 6 lines *read* 5, at the most 6 lines.
— 435, — 2 from bottom, *for* $(5{\cdot}62)^2$ *read* $(5{\cdot}6)^2$.
— *ib.* — 1 from bottom, *for* 900 *read* 9000.

ERRATA IN VOL XI.

Page 4, line 22, *for* As induction cannot *read* As conduction cannot.
— 6, — 9, *for* conduction *read* induction.

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LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

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[FOURTH SERIES.]

JANUARY 1856.

I. *On the Action of Non-conducting Bodies in Electric Induction.* By Professor FARADAY *and* Dr. P. RIESS*.

THE accompanying letter explains itself. I have received a most kind answer from M. Riess, which I wish added to it. I have altered the English of the answer a little, chiefly in single, small words, and only in those cases where I thought the alteration would make the author's meaning more clear. Certain expressions of M. Riess almost ask for a reply. In respect of these cases, and to remove ambiguity as to my own meaning, I have ventured to add some foot-notes; but I trust they will be received, not as exciting discussion about hypotheses, but simply in explication (to the reader) of my own view. It is not the duty or place of a philosopher to dictate belief, and all hypothesis is more or less matter of belief; he has but to give his facts and his conclusions, and so much of the logic which connects the former with the latter as he may think necessary, and then to commit the whole to the scientific world for present, and, as he may sometimes without presumption believe, for future judgment.

MY DEAR M. RIESS, London, Nov. 19, 1855.

I have only just arrived at the knowledge of a paper written by you on the action of non-conducting bodies in electric induction; for though I had seen it in Poggendorff's *Annalen*, I could not read it. A translation has, however, appeared in the Philosophical Magazine for June of this year, vol. ix. p. 401,

* Communicated by Professor Faraday.

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and by it I find that I have failed to convey to your mind (and therefore, perhaps, to the minds of others) my true meaning; so that what you think to be my view, is in some very important points absolutely the reverse. You will not wonder that I am anxious to set myself right in such a matter with one who holds your high position in science. For that purpose I must refer to the pages of the Philosophical Magazine; for though I am not a judge of the strictness of the translation, I have no other means of access and reference to your paper.

At the bottom of page 402, the paper says that Faraday has endeavoured to establish the notion that "induction is not produced by the action of electricity across space, but that an electric body acts *only* on the contiguous particles of an insulating medium," &c. If you refer again to my papers, you will find that in the very beginning of that on induction (1165.) I have especially limited the cases to those of *ordinary induction, i. e.* cases where matter is present; at (1215.) this expression is repeated; and again in vol. ii. Exp. Res. p. 267. Instead of saying that induction cannot occur across space, I have especially spoken of the case of a vacuum (1613–1616.), which case is enlarged upon in a letter to Dr. Hare, vol. ii. Exp. Res. p. 262. 266.

At p. 403, Phil. Mag., your paper says, "It follows from other experiments made by Faraday (1218.), that the induction would have been diminished had a conducting plate been introduced between the two; for, *according to Faraday's opinion*, the introduction of the conducting plate would have caused the induction to take place in curved lines around the edges of the plate, instead of in right lines through the intervening stratum of air." If this translation conveys your meaning, then I cannot find out what expression of mine has led you to suppose the above is my opinion. I have nowhere said or implied that the interposition of such a plate would have diminished the induction, or made it take place in lines only round its edges, or more curved than before. On the contrary, I know that because of such a plate more lines of force would have passed to the space occupied by it than before; that as far as regarded that portion of space, induction would be replaced by the better function of conduction; that instead of interfering with induction, it would have favoured the final result, although that result would be complicated by the form and size of the plate, the distances of it and the acting bodies, and by other circumstances, as your paper well shows.

The case of mine to which your paper refers as above (1218.), is one of those which I sought for as establishing the *possibility* of induction in curved lines, and is not given as a proof that it must *always* be in curved lines, which is very far from my

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thoughts. In it the metallic piece (ball, hemisphere or plate) referred to is *uninsulated*, not insulated (1218–1230.). It is also the conductor upon which the induction terminates, and not a conductor interposed in the course of the induction; cases so different, that much of the reasoning which belongs to one has no relation to the other. The latter case is not specifically referred to in the Experimental Researches, because I thought it thoroughly well known, but it is given in my letter to Dr. Hare, vol. ii. of collected papers, p. 263.

Perhaps the following mode of putting the matter will make my views on this point clear to you. Let P be an insulated charged body, inducing upon N an uninsulated metallic body, *np* being at first away. Then let *np* be introduced, being a non-conductor equal to shell-lac or sulphur, but of the same specific inductive capacity as air; no change of the disposition of the forces will take place, for the particles of *np* will be polarized just as the particles of the air displaced by it were. Then consider *np* to be endued with conducting power, as if it were converted into a metal; its particles will now discharge to each other; the parts at *n* and *p* will be more negative and positive than they were before, because the sum of induction distance between P and N is shortened by the diameter of *np*, and so the induction is stronger; and instead of the lines of force from P to N passing round *np* (as your paper makes me to say), *more* will fall upon and pass through the space *np* now that it is a conductor than before when it was an insulator (1826. 1337. 1338.). I am sure I need make no further reference to these points, for I am satisfied that when you look at the paragraphs 1218. to 1230, and perhaps also to vol. ii. Exp. Res. p. 279–284, you will at once see what my meaning was, and what my views are. The results according to them are precisely such as you describe at pp. 406, 407, Phil. Mag.

In your paper (Phil. Mag. p. 410), you describe an experiment which I know well, and consider as one of the strong proofs of the truth of my views. A plate of shell-lac is placed with its anterior face 12 inches from the positive knob of an electrical machine, and its posterior face 1 inch from the flame of a spirit-lamp, and then moved about before it; when taken away and examined, the anterior face is found by you charged negative, and hence you conclude, that, prior to the discharge of the posterior face by the flame, the induction had rendered the anterior face of the shell-lac negative, and the posterior face positive, just as would have happened with a metallic plate, and as far as I understand your paper, by a like act of conduction

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through its mass. Now my view of the induction agrees with yours as respects the anterior and posterior faces of the shell-lac plate; but it differs in two important points: it assumes that if the plate be supposed to consist of an infinite number of parallel plates, each composed of a single layer of particles, each plate has its anterior negative, and its posterior positive surfaces; and that the outer posterior positive surface is not the consequence of the transmission of electricity by the intervening *conducting* particles between it and the anterior negative surface, but of a transmission of the force by the polarity of the *insulating* particles. Upon so stating the case, one or two considerations arise fitted to test the relative value of the two views, and as yet they confirm me in mine.

If the shell-lac plate had had its anterior surface charged negatively, as the like surface of a metallic or conducting plate would have been, then that surface should not have remained exclusively charged on the removal of the plate from the induction; the shell-lac plate, like a conducting plate, should have been found charged over both faces and all its surface; for the same conduction which would permit the flow of electricity under induction, would permit the return to all parts when the induction was removed. As induction cannot be assumed for one part of the experiment and refused for the other, so I find this consideration alone fatal to your view, as I understand it from the translation.

The second consideration is of this kind. If the shell-lac plate whilst in the inductive position be considered, according to my view, as a mass of non-conducting particles polarized, then the action of the spirit-lamp flame will have been to convey, by convection, negative electricity to the posterior surface of the plate, to neutralize for the time its temporary constrained induced positive state; and it is *that* surface which (after the removal of the plate from the induction, and the return of the constrained state now no longer sustained by P) is to be considered as negatively charged, and not the anterior surface, the latter now being only held in a relatively negative state by the still remaining polarity of the particles between it and the really charged posterior surface. So, apart and beyond the argument derived from conduction, other determining considerations may thus be raised. If your view be the correct one, it is the anterior surface only which is charged negatively, and that by an inductive action half discharged; in my view, it is the posterior surface which has that state conferred on it by *convection* from the flame:—in your view the inner parts of the plate are in their natural condition; in my view they are still polarized, being retained in that condition by the posterior negative charge. Happily the question

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whether it is the anterior or posterior surface which is negatively charged, may be solved, though not by the indifferent approach of either side of the plate to a gold-leaf electrometer; for with either side, indications of negative electricity will be obtained; and if the excited surface be in both cases at equal distances from the electrometer plate, the action will be greater (because of specific inductive capacity) when the body of the shell-lac plate intervenes between the cap and the excited surface than when air only is interposed.

To make these points clear, once for all I will describe the plates I have used; and for easy reference to position, will give a diagram of the forms of experiment. One plate was of shell-lac, $4\frac{1}{2}$ inches square and 0·9 of an inch thick; the other was of sulphur, 5 inches square and 0·8 of an inch thick. A strong white silk thread was made fast round the edges of each plate, and then a long loop of the like silk being fastened at the two corners of one side-edge, and a like loop at the two corners of the opposite side-edge, the two loops served as handles by which the plates could be insulated, and yet carried about or applied in any position to the electrometer. In the figure, S is one of these plates supposed to be seen edgeways; P represents the inductric or originally charged body, and N (whether flame, point, hand, or ball) the inducteous body; between these two, P and N, that induction takes place to which the plate S is, as far as regards the results of the experiment, subjected, and the effects of which are to be examined. The results were precisely alike with both sulphur and shell-lac plates. If P was made negative, they were also the same, but with inverted signs. I will describe those obtained with the shell-lac, and will always call that face towards P the anterior, and the face towards N the posterior faces of the plates.

Making P the end of the positive conductor of an electrical machine, and N an uninsulated metal ball or plate, then the shell-lac plate was put into its position, retained there for thirty seconds or more, was removed, examined by a gold-leaf electrometer, and found perfectly free from charge on either face or any part. The shell-lac plate was then restored to its position in the induction, and N made a spirit-lamp flame applied in the manner you describe. The shell-lac being taken away, was examined by laying the plate without friction on the cap plate of the electrometer. The shell-lac was found to give strong negative charge to the leaves, whichever face was on the cap; but the signs were

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much the strongest when the posterior face was in contact with the cap, showing, so far as that went, that the charge was really on that face.

According to my view of induction, that face of the plate had been charged negatively by the flame; for the portion of the induction between P and the flame could be destroyed by the convection dependent on the latter where air only intervened between it and towards P; but as the air in the direction of the conduction terminated at the posterior surface of the shell-lac, so the flame could convey its state of charge to that surface only, the insulating power and solidity of the shell-lac preventing further changes in that direction; hence the result already described. As the flame had power to charge the posterior surface, so it can discharge it, and accordingly by moving the flame for a moment parallel to that surface, and about an inch from it, the plate will be entirely discharged. The previous state of negative charge on the posterior surface of the plate will, if wished, remain for a minute, or two minutes, or even five or ten; and yet the momentary use of the flame discharges it entirely. The result accords perfectly with my view, but, as it appears to me, is entirely opposed to yours. Nor can I see how the assumption of any degree of conduction in the shell-lac, compatible with the acknowledged facts dependent on its insulating powers, can explain the result.

But it may be said, that the second application of the flame, instead of discharging the negative posterior surface, has really charged it positively to an amount equal to the supposed negative charge on the anterior surface, and so covers the effect of the latter more or less according to the thickness of the plate; and then the question is, is the plate entirely discharged, or is it now doubly charged, *i. e.* with one surface positive and the other negative? I find it to be *entirely* discharged; for if I place either surface on the cap plate of the gold-leaf electrometer, and then carefully bring an uninsulated metallic plate to the other surface, I find no effect on the electrometer; whereas there would be such an effect if the plate had been charged as a Leyden jar.

Or it may be supposed that the second application of the flame, though applied on the posterior side of the shell-lac, has somehow or other discharged the negative anterior surface. This is easily shown not to be the case, by the application of the flame on the anterior side, and then still stronger proofs than those already obtained appear against your view and for mine; for according to my view, such an application of the flame ought to cause the anterior face to acquire a positive charge, inasmuch as a second case of induction is set up, in which the posterior negatively charged face of the shell-lac is the inductric body, to which

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the flame plays the inducteous part as before, and by its well-known powers of convection transfers its state of charge to that surface of the shell-lac (formerly the anterior) now opposed to it; whereas on your view it ought to be simply discharged. The shell-lac plate was therefore placed before the charged body P, and the flame moved about before its posterior surface; then the plate was taken out of the inductive position and the flame moved before its anterior surface; after that it was examined by the electrometer. When the anterior face was on the cap plate of the instrument, the latter indicated a positive charge; when the posterior face was in contact with the cap, the instrument indicated a negative charge, being the same kind of electric charge for that face as before, but much weaker. The apparent weakness ought to occur, for now the negative charge of the posterior face is exercised inductively through the shell-lac towards the positive charge of the anterior face, and *vice versâ*; and this was proved to be the case by bringing the hand or an uninsulated metallic plate towards the upper anterior surface, whilst the posterior surface was in contact with the electrometer cap; for the negative divergence of the gold leaves then increased very greatly, the negative electricity being set free to a large extent from the induction of the positive anterior surface. And when the positive anterior surface was in contact with the cap of the electrometer, its highly charged condition could be exhibited in like manner. So the flame, carefully approached, can only discharge the side which has received a charge, and that only if approached on that side; if brought opposite the other side, it conveys to it the opposite electricity and leaves the plate doubly charged.

These experiments are by no means difficult or delicate, and are easily made in the most convincing and varied manner (a few simple precautions being taken), but always with the same results. P or the inductric body is best if of large surface. An excited glass rod, or, better still, an excited plate of gutta-percha (a shoe sole), are very good for the purpose; either may be brought to within an inch of the shell-lac or sulphur S, and still communicate no charge to it if discharging conductors be not near. A fine metal point may be used at N instead of the flame; or even conductors and contact be employed, as in the following manner. A sheet of gold-leaf was laid on the cap plate of the electrometer; P was put into place, and also the shell-lac or sulphur plate S, and the flame applied for a moment at N; then the plate S was removed and placed with its negatively charged posterior surface in contact with the gold-leaf on the electrometer; immediately the latter showed a strong divergence (often more than the instrument, though very large, could bear); but

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besides that, if an uninsulated wire was brought towards the cap or gold-leaf, the charged posterior surface was discharged with a spark, and the electrometer and shell-lac were left perfectly uncharged. It is but a very small step to coat the posterior surface with tinfoil beforehand, and then all the experiments can be repeated, using contact with an uninsulated body instead of the flame. Another step led to the coating of the anterior surface; the induction within the shell-lac between these surfaces and perpendicular to them being precisely of the same kind as if these coatings were away. If S, or the inducteous body, be made one that cannot at a distance communicate a charge to the posterior coating, being, for instance, an uninsulated metal ball or plate, then each of these coatings has for the time a polar condition like that represented by *np* in the first diagram of this letter, *i. e.* their anterior surfaces have negative charge, and their posterior surfaces positive charge, when a positive inductric body P is employed, and so long as the induction continues.

I think you doubt the existence of specific inductive capacity. You obtain the effects which I refer to it, but seem to explain them by some act of conduction in the shell-lac, like that in interposed metallic plates; indeed, by the same act as that which you suppose confers the assumed negative state on the *anterior* surface of the shell-lac plate. Now if any of the induction effects be due to such a conduction, this latter quality ought to appear in very numerous and various forms of experiment, especially if *time* be taken into account. I have taken the plate of sulphur, set it before P, applied the flame before the posterior surface, removed the plate, applied the flame before the anterior surface, and thus charged the sulphur negative and positive on the two sides, as before described, in less than four seconds, and to a considerable degree. That charge, thus quickly gained, the sulphur has retained apparently unimpaired for several minutes, and at the expiration of several hours it was still strongly charged. Now how could any conduction within the mass of the sulphur (of the nature of that which occurs in metals) have caused the appearance at its surfaces of the two electricities in a moment or two, and to twice the amount of what would have been evolved if air had been there, which conduction was yet not competent to effect their return in a period many hundred times as long? We have reason to believe that induction is sensibly instantaneous; for if we take the sulphur plate coated over the middle part of each face, and place a large metallic ball or plate for P opposite to it, three successive contacts, one to touch P and charge it, the second to touch for an instant the coating on the posterior surface of the sulphur, and the third to touch P and discharge it, are sufficient to put on the full inductive state

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through the sulphur and secure the resulting charge. By the use of a finger key these contacts can be made in the fiftieth part of a second, and by a little mechanical arrangement even much quicker; yet as far as I can find, the coated sulphur surface is as fully charged in this brief period as if the induction had been sustained for a minute or an hour. How are we to conceive that any degree of conduction of the sulphur consistent with the prolonged insulation which can follow, can have concurred to this brief and complete act?

The foregoing results appear to me to be crucial in their character, and to leave no question open as to the possibility of the action of interposed insulating matter being of the same nature as the action of interposed conducting matter in cases of induction. I would go further into them in explanation and illustration of my own views, and of the truthfulness of specific inductive capacity, if I thought it necessary; but I should have little more to do than repeat what is already said (and that many years ago) in the Eleventh Series of the Experimental Researches, and so I refrain.

The effect you mention at the bottom of page 404 and top of 405, Phil. Mag., is to me a very natural result of the high specific inductive capacity of shell-lac. In one place you say, in relation to it, that "no reason can be assigned why a *small piece* of shell-lac," &c.; but I cannot consent to accept that as a small piece which is in reality a small portion, not separated, of a large piece; as I could not say, that that was a small piece of metal which is only a small part of a large plate. A greater inductive capacity disturbs the lines and distribution of force in a manner equivalent to a certain amount of conductive power; and yet the two effects may be perfectly distinguished by such experiments and reasoning as that I have just applied to the examination of the condition of the shell-lac plate.

You will see, my dear Sir, that I am anxious to stand rightly before you; at the same time I would not have presumed thus far if I had not believed that there was some great misapprehension in your mind as to my opinions. You will perceive, also, that I find no reason to change any of my views of static induction as set forth in Series XI. I must confess, that as your paper has appeared in Poggendorff's *Annalen* and in the Philosophical Magazine, I should not like the case to remain before the scientific world just as it is, as it might be thought I acquiesced in the statements there made; and if I might suppose it would not be disagreeable to you I would put this letter into the Magazine, unless, indeed, you preferred some other mode of communication with the public. In the mean time I shall send it to you; and as many months have now elapsed since the publication here, I

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hope you will give me an early note saying whether you object or not.

I am, yours very truly,

Prof. P. Riess, M. Faraday.
&c. &c. &c.

My dearest Sir, Berlin, December 10, 1855.

In replying to the letter with which you have honoured me, I must at first claim your greatest indulgence for my English. I mean not the errors which are easily corrected, but the improper choice of words, which in theoretical controversies is of consequence, and which I have no hope to avoid. Before I enter into the discussion of your remarks concerning my paper on induction, it may not be improper to say a word upon the old theory of static electricity.

It appears to me, that a theory of a branch of the experimental sciences should be deemed good, and not be abandoned, so long as it is sufficient to account for all facts known by applying a simple principle, be it paradoxical or not, and so long as it comes not in contradiction with itself, or the theory of a congenial branch. The old theory of light has been abandoned, not because its principle of the emission of myriads of particles of light, endued with the greatest velocity and many perplexing properties, was highly paradoxical, but because it was found incompetent to account for the great class of phænomena of diffraction and polarization. I see not the like in the old theory of electricity. It assumes, indeed, the action at a distance, and I agree entirely with you that such an action is extremely difficult to conceive; but admit we not the like in the great theory of gravitation? and admit you not also this action in an extraordinary case of induction in electricity? The action at a distance consists here in the attraction of electricity of one kind, and the repulsion of the other in every particle of matter, and is unlimited; that is to say, if an electrified particle E acts upon a particle of matter A, and a particle of matter B is placed anywhere, the action of E upon A is not hindered nor weakened, and exists in the same amount as before. These premises granted, the theory accounts for the phænomena of static electricity in the simplest manner. All these phænomena are instances of the arrangement of electricity upon the surface of bodies, and their arrangement is made dependent upon the equilibrium of a number of forces which the electric particles exercise mutually on each other. Thus the electrostatic problems are changed into problems of pure mechanics, and the principles of this science find their application. The advantage of this method is very great; it gives the result of each experiment as the sum of single actions,

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which the mind conceives without difficulty, and leaves to mathematics the pains to sum up the single effects and to give the amount of the sum. If their summation is often too complicate to be completely effected, I think that not a fault of the theory, especially as it is in most cases not difficult to imagine, by means of general considerations, the final result. Therefore I have long ago defended this theory against its—indeed not very dangerous—antagonists, and I could not abstain from continuing the defence, when an adversary arose in the man whom I venerate as the greatest natural philosopher of the age.

Upon your first remark I reply, that, writing on a case of induction in air, I gave your opinion on that induction, and avoided intentionally to mention your opinion on a case which had not occurred; for had I mentioned it, I would have been forced to add, that you admit solely a *limited* action at a distance*, and to explain, that this presumption in respect to the case in hand is of the same consequence as if you denied that action altogether.

In respect of the reproof made in the second remark, that I have misrepresented your meaning on the action of the conducting intermediate plates in induction, I must be the more anxious to disculpate myself, as, if I am not mistaken, this disculpation hits the very root of all differences between your theory of induction and the old one. I have said, it follows from your experiments that the introduction of a conducting plate between an inductric and an inducteous body would have diminished the action of the former upon the latter, because this action, according to your opinion, would pass in curved lines instead of in right lines through the air. In the experiments referred to, a rubbed shell-lac cylinder, and in contact with it an uninsulated metallic disc, had been employed, and a *fact of proof* is given (Exp. Res. 1221), "that the induction of the shell-lac acts not through or across the metal." This fact of proof consists in the observation, that a carrier ball receives inducteously no charge, or a weak one, if it is applied to the centre of the upper face of the disc, where the carrier is nearest to the inductric, and no straight line can be drawn between both except through the metal; and the observation that the carrier receives a strong charge in the air at some height above the centre of the disc. Hence you conclude "that the induction† is not through the metal, but through the surrounding air in curved lines." I thought myself entitled to presume that you would make the same conclusion from the same fact of proof in experiments of a varied form, and

* My view puts no limit to the action which is not paralleled in the case of light; where matter is, it is included in the action; where it is not, the action is considered as going on without it.—M. F.

† On the further side, the metal being always uninsulated.—M. F.

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I thought it the more, as I saw no other way to account for this fact according to your theory. When the rubbed shell-lac cylinder is replaced by an electrified metallic globe, and a sufficiently large metallic plate is placed at some distance above the globe, the carrier receives only a weak charge from the centre of the upper surface of the plate (which is not in sight of the globe), and an increasing charge if it is raised. If the plate is insulated, the carrier must not be applied to the plate, but the charge increasing with the elevation of the carrier above the centre, and the maximum of it in a certain height, is still remarked. Hence I conjectured that you would consider the action of a metallic intermediate plate between an inductric and an inducteous body as *screening* the latter from the induction in straight lines of the former*; and I was confirmed in my conjecture by § 1681†, where you say "that the electric power is limited and exclusive." Surely you will find this conjecture not to be an unfounded one, if you call to mind that the philosophers who have adopted your views on induction have made the same. Melloni has believed to screen his electroscopes from the induction of a conductor by the interposition of a metallic plate between both; and De la Rive relates with the same meaning the experiments with your differential-inductometer in his *Traité d'Electricité*, vol. i. p. 131 (of which an English edition exists, which I have not seen). He says, "Si on interpose une lame métallique soit isolée, soit mieux encore, communiquant avec le sol, entre A (the positively electrified inductric plate) et B (the inducteous, which has been touched before), aussitôt B donne des signes d'électricité négative très forte, qui proviennent de ce que *l'induction cessant d'agir* sur elle, etc. Ainsi, mettre un disque métallique entre A et B, cela revient a remplacer B par un autre disque plus rapproché de A qu'il ne l'était, et par conséquent le *soustraire à l'induction* de A."

The metallic intermediate plate, insulated or not, is here said to have withdrawn a body from the induction; it is regarded as a screen which intercepts the electric induction; as an opake body intercepts the light. I am extremely gratified that you partake not of this view, but I must confess that I cannot see how to account, by the manner exposed in your letter, for the results which I have obtained with intermediate conducting plates. Let P be the originally electrified globe, N the unin-

* If uninsulated, yes; if insulated, no;—as regards the final result of all the actions (inductive and conductive) on the inducteous body.—M. F.

† "(1681.) A striking character of the electric power is that it is limited and exclusive, and that the two forces being always present, are exactly equal in amount. The forces are related in one of two ways: either as in the natural normal condition of an uncharged insulated conductor, or as in the charged state, the latter being a case of induction."—M. F.

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sulated globe induced, *np* the metallic insulated disc (edgeways seen), so interposed that the line joining the centres of the globes passes perpendicularly through the centre of the disc. According

to your view, the faces *n* and *p* of the disc are more negative and positive than when the disc was of atmospheric air (*i.e.* the metallic disc away); and the induction on N must be stronger than before. But really this is not always the case: the induction on N appears strengthened or diminished, according as the intermediate metallic disc is small or large, is thick or thin. I am not able to find by your reasoning what difference should occur when the disc *np* with the same diameter has a thickness of 0·25 or of 0·04 of an inch. It seems to me, that in both cases the electric state of the faces *n* and *p* should be greater than with the air-disc. However, with the thick metallic disc the induction on N appears greater, with the thin disc less, than in the case where no disc is present (p. 408 of my paper)*. When the thick disc is employed, and therefore the induction on N appears strengthened, if we touch the disc for a moment, and insulate it again, the induction on N is diminished. If we say that the exalted state of *p* is taken away by momentary touch, it is to be expected that this state be fully restored at the moment when the disc is again insulated†. Be it as it may, I am not aware that your theory admits the action of an inductric and an inducteous body upon a third body to be independent of each other, and that is, I believe, the essential point in which the two theories differ thoroughly‡. The old theory accounts in the simplest ima-

* The induction of P is in my view not exclusively upon N, but upon all surrounding bodies, even to the walls of the room. When the metallic insulated disc *np* is changed in size, the distribution of the induction is changed with it. A small plate, because of its thickness of conducting matter, lessens the electric resistance between P and N, and the induction on the latter is increased. A larger plate of the same thickness, or even thicker, may diminish the induction on N by a redistribution of the forces; more induction upon surrounding bodies now taking place, because of the extension of its periphery towards them.—M. F.

† I do not expect any restoration of the previous state of the disc, and believe I know that it will *not* occur. A momentary uninsulating touch instantly brings on a new state of the induction and of the plate, which is final and remains after the uninsulating contact is removed. The only disturbance of this state is that due to the presence of the uninsulating wire which, whilst it is there, takes part of the induction on to itself, and that caused by gradual discharge due to moisture and dust of the air and to imperfect insulation.—M. F.

‡ The question to my mind is, Is the effect in the shell-lac plate *np* dependent or independent of internal conduction amongst its particles? if independent of internal conduction, what is it dependent on, apart from the

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ginable manner for all cases here concerned. It presumes that the three electric strata—upon the surface of the globe P, upon the face *n*, and the face *p*—act independently of each other, inducing upon the globe N. If we denote with $f(P)$, the inductive effect of the globe P on the globe N, with $-f(n)$ that of *n* (the sign − says that the effect is contrary to that of P), and with $f(p)$ the inductive effect of the face *p*, the theory asserts that in all cases the final effect on N is dependent on the amount of the sum $f(P)-f(n)+f(p)$, and leaves it to the calculation to say if the sum is greater or less than $f(P)$. In the sole case where $f(p)$ vanishes, that is to say, when the intermediate metallic plate has been touched or is uninsulated, it can be said without computation that the sum of inductive effects of globe and plate is less than the effect of the globe alone.

After having experimentally shown, that with conducting intermediate plates the induction can be strengthened as well as weakened, and with non-conducting plates weakened as well as strengthened, I ventured to advance the opinion, that the action of plates of whatever nature have the same cause, viz. the arrangement of the electricities of opposite kind upon the surfaces of the plates. I examined roughly (if necessary, it could be made very accurately) the arrangement of the electricities upon a metallic disc, and I concluded that the electricities are arranged in a similar manner (not the same) upon a non-conducting disc. I concede that this conclusion is not unobjectionable, but I maintain that it must be made necessarily at first, and cannot be abandoned unless it is proved false. The simple fact, that a non-conducting body is attracted by an electrified body, shows clearly that the non-conducting matter as well as the conducting is instantaneously provided by induction with both electricities.

As a more direct proof of this induction upon insulators, without an essential connexion with the subject I treated of, I have described an experiment which occurred to me and which I had nowhere found. To this supplementary experiment belongs your third remark, to which I proceed. A shell-lac disc is quickly moved once from above to below between a flame and the globe of the conductor of an electric machine (not "to and fro;" I have indicated this error of translation to Prof. Tyndall in a letter dated 19th June)*. The anterior face of the disc is found to be strongly negative. You agree with me, that without the flame both faces of the disc have been instantaneously provided

polarity of the particles which I assume as the cause? Or again, how can conduction and insulation, considered as contingent causes, give as their result the same distribution of force?—M. F.

* I would observe that I am not responsible for the translation, which, however, on the whole, appears to be excellent.—J. T.

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by induction with negative and positive electricity, but you differ from me in respect to the explication of the experiment, in respect, as you say, to the manner by which the disc has been electrified, and the part which the flame has acted in the final result. As to the first point, a mistake must have happened, inasmuch as I have nowhere mentioned my view on the manner by which the conducting and non-conducting bodies are excited by induction. I fear that the word "distribution," whereby the German word "Anordnung" (which signifies "arrangement") has been translated, has caused the mistake. It is said (p. 412), "there is no essential difference between the actions of conducting and non-conducting bodies, but inasmuch as the distribution of electricity upon them," &c.; and further, "in conducting bodies the distribution of electricity," &c. (*ibidem*). My meaning is this: It is admitted that each intermediate plate, be it of conducting or of non-conducting matter, is by induction instantaneously provided with both electricities, which are arranged in a certain manner upon both faces of the plate. Upon a conducting plate I can specify by examination in every case the arrangement of the electricities, and thereby account for the action of this plate upon a body in its vicinity and induced by an electrified body. Upon a non-conducting plate I cannot examine the arrangement of the electricities, but with a presumed arrangement I can also account for the action of this plate, and therefore I must deny an essential difference to be between the action of conducting and non-conducting bodies in electric induction*. That the manner whereby the induction is produced upon a conducting and a non-conducting plate is alike in every respect, I have neither said nor meant.

As far as regards the result, indicated in my paper, of the experiment with the flame, it is neither uncertain nor equivocal,

* Suppose a fluid insulating medium to exist between P and N instead of air, and the solid interposed plate *np* to have like insulating power and inductive capacity as the medium, as for instance shell-lac in camphine or solid sulphur in melted sulphur, are we to expect the two electricities to appear at the surfaces only of the solid plate, and not, as I suppose, in every possible section of either the fluid or the solid by planes, which, being parallel to the surfaces of the plate, or rather perpendicular to the lines of induction, may be supposed to pass between the particles and mark their separation one from the other? Would not the first supposition be, to attribute to insulating solids a power denied to insulating fluids, and would it not also be equivalent to an admission that the solid could acquire a polar state under induction which yet would be denied to its particles? As the phænomena of specific inductive capacity are now admitted, it is very desirable that "the old theory" should state how it accounts for them in unexceptionable cases, as with sulphur or shell-lac; and also explain how a non-conducting plate can have the opposite electricities evolved and located upon its two surfaces without conduction; or without what I have called polarization.—M. F.

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The shell-lac disc was moved only once from above to below between the positively electrified conductor of a machine and a spirit-flame; the anterior face of it was laid with sliding contact* on the knob of a gold-leaf electroscope; the disc was withdrawn and the electricity in the electroscope examined. Always negative electricity was found, weaker or stronger; the strongest, when the centre of the large disc had touched the knob, and it was carefully breathed upon, whereof the reason is obvious. I have imputed to the flame the essential part of destroying the positive electricity of the posterior face. You have observed the fact that the posterior face is negatively excited, and you have hence drawn some consequences concerning the mode of induction on the plate which I cannot admit. The fact of the posterior face being negatively electrified appears to me a very complicated one, and resulting from one of the two following causes, perhaps from both:—First, the flame is inducteously excited by the originally electrified body and imparts its negative electricity to the posterior surface; secondly, the negatively electrified anterior face of the disc acts by induction upon the posterior face. Concerning the first assumption, I have concluded from experiments made on the electric properties of burning bodies (Poggendorff's *Annalen*, vol. lxi. p. 545), that a flame electrified by induction acts upon a body in its vicinity by means of its electricity, which is contrary to that of the inductric body. As to the second assumption, a decisive experiment, as it appears to me, has been made and described by me in vol. i. § 300 of my work on Electricity. A shell-lac disc was held, by means of a handle, freely in the air, and rubbed upon one surface (we will say, the superior) strongly with fur. Although it cannot

* I gave the one motion between the inductric body and the flame, and obtained precisely the same results as those described in my letter. It is quite easy to ascertain which surface of the plate *np* is charged, and whether positive or negative, without ever making contact with the ball or cap-plate of the electrometer, by a near approach only. I believe it to be essentially necessary to avoid a sliding contact between the shell-lac plate and the metal ball of the electrometer, for I find that by employing a perfectly uncharged plate and instrument and making such a contact, electricity is excited, the shell-lac becomes positive, and the metal negative, so that the moment the shell-lac is withdrawn the electrometer diverges with negative electricity. When a charged piece of dry shell-lac, made positive by friction with metal and to a degree enough by near approach to diverge the gold-leaves of an electrometer an inch or more, is employed, I find it impossible to convey that charge to the dry instrument by friction against its metal cap; the shell-lac only becomes more positive, and leaves the instrument in the negative state: therefore I doubt the simple communication of negative electricity from weakly-charged, dry, insulating shell-lac to dry metal by friction contact; though I expect in every case excitement and evolution of electricity, and that the electrometer will be rendered negative and the shell-lac positive.—M. F.

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be doubted that the superior face was negatively excited, the inferior face was found also negative. On the contrary, had the shell-lac disc lain upon an uninsulated metallic disc during the rubbing, and after that the negative electricity of the superior face was destroyed by the application of a flame (or the touching with a metallic plate, as instantly will be seen), the inferior face was found to be positive. After destroying this positive electricity, the superior face was again negative, and thus continuing, alternately one face could be made positive, the other negative. This experiment gave me the means to obtain easily an electrophorus with a positively excited cake. For that purpose the cake was laid on its uninsulated metallic mould, strongly rubbed with fur and inverted in the mould, so that the not-rubbed face was uppermost. When this cake was covered with its covercle (a metallic disc), I had an electrophorus which gave negative electricity, instead of the common electrophorus giving positive electricity.

With respect to your fourth and last remark, I concede entirely that it is not correct to consider a small portion of a large piece of shell-lac as equal to a small piece of the same, which I have done at p. 405 of my paper. But I believe to have rendered this incorrectness innoxious by referring to the end of my paper, where I have explained why a partial introduction of the non-conducting plate between the inductric and the inducteous body apparently diminishes the induction and strengthens it by complete interposition. I still consider this opposite effect of one and the same plate, together with the fact that the placing of the plate at the side of the inductric body increases the induction (p. 411 at the bottom), very difficult to be explained by your theory of induction.

I have little hope to persuade you, my dear Sir, to modify your views on the action of insulators in electric induction, and, I confess, if I could I would scarcely wish it. The great philosopher works best with the help of his own conceptions, his self-made tools, whose imperfections he avoids by dexterous application. But these tools, so efficacious in his hand, are not only useless but very dangerous in the hands of others, and you know what mischief, for instance, the conceit of electric screening has lately done in the hand of the since deceased Italian philosopher. You will therefore not blame me if I follow the publication of your remarks by my reply. I cannot have any objection against the mode and place which you choose for this publication, and I know that immediately after the appearance of your letter Prof. Poggendorff will give a translation of it in his 'Annals.'

I am, my dearest Sir,
Yours most faithfully,
P. Riess.

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II. *On the Deportment of Bismuth during Solidification.* *By* R. Schneider *.

During the change of bismuth from the fluid to the solid state of aggregation, it often happens that the surface of the apparently solidified mass is broken through in one or more places by the liquid metal underneath, which soon solidifies in globular forms. This phænomenon is generally accepted as a sure proof that bismuth at its moment of solidification experiences a considerable expansion. Marx, indeed, has determined the expansion from the magnitude of the globules thus forced through the metallic crust, and found that they amount to $\frac{1}{33}$rd of the entire mass. The above proof is not correct; pure bismuth, whatever may be its expansion at the moment of solidification, does not exhibit the squeezing out of the globules of metal. This phænomenon is observed with impure bismuth only, and it is a remarkable fact that the bismuth thus squeezed out is found to possess a high degree of purity, even when the metal made use of contains a considerable quantity of foreign matter. The following experiments will serve as a confirmation to this statement.

1. In a mass of bismuth sold to me as pure, which, however, during solidification showed the protrusion of the metallic spheres, 2·5 per cent. of impurity was found upon analysis; chiefly consisting of sulphur, a little copper, and a trace of iron. From 64 grammes of this metal, by repeated melting and pouring out upon a clean plate of porcelain, 32 grammes or 50 per cent. of metallic globules were gradually collected. These globules were melted altogether, and the mass again poured out; during solidification *no* globules of bismuth appeared in this case. The analysis of the mass showed that out of 100 parts 99·92 were bismuth. From sulphur, copper, and iron the mass was totally free.

2. 100 grammes of almost pure bismuth were melted with 3 grammes of sulphur, 1 of copper, 0·25 of a gramme of silver, 1 gramme of nickel, and 1·25 gramme of arsenic; that is, the substances with which the commercial bismuth is most usually associated. Although at the high temperature necessary to melt the mass a portion of the sulphur and arsenic was lost, the foreign constituents could nevertheless be rated at 5 per cent. at least. From this metal, heated as before, 25 per cent. of spherical bismuth was separated. The latter being melted and suffered to solidify, yielded *no* globules. The analysis showed that this mass contained 99·78 per cent. of bismuth, 0·11 per cent. of silver, and a trace of sulphur. Copper, nickel, and arsenic were not to be found. It is worthy of remark that when silver is contained

* From Poggendorff's *Annalen*, No. 11. 1855, p. 494.

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in raw bismuth, a portion of the silver accompanies the metal protruded during solidification.

3. To the metals already mentioned lead may be added as an associate of commercial bismuth. 50 grammes of pure bismuth melted with 2 per cent. of lead gave out no spheres on solidification; this immediately took place when 2 per cent. of sulphur was added to the mixture. The spheres protruded to the amount of 20 per cent.; when melted and cooled they showed an even surface, and mere traces only of sulphur and lead were to be found in them.

4. From a mixture of 80 per cent. bismuth, containing some silver, and 20 per cent. sulphide of bismuth, some large metallic globules were protruded during solidification. These were found to contain 99·69 per cent. of bismuth, and 0·11 per cent. of silver. Sulphur could not be detected in the globules, although the original mass contained almost 4 per cent. of the substance.

5. Chemically pure bismuth, which, when melted by itself yielded *no* spheres, was melted with 5 per cent. of tersulphide of bismuth (containing therefore nearly 1 per cent. sulphur); at the moment of solidification bismuth spheres free from sulphur made their appearance.

These experiments show that the phænomenon of globules making their appearance on solidification, is not exhibited by pure bismuth, but only by such as contains foreign ingredients. Of the latter sulphur appears to be the chief cause of the phænomenon; at least I have never observed it when the bismuth was rendered impure by the heavier metals alone; but always when, together with such, sulphur was present, or when it alone was the cause of impurity. These experiments show further that the protruded metal possesses a high degree of purity; it may, indeed, be regarded as almost chemically pure.

These effects may be simply explained in the following manner. The binary compounds of bismuth with the foreign substances, particularly the sulphide of bismuth, solidify sooner than the bismuth itself; and inasmuch as those compounds, without doubt, expand [contract ?] by solidification, a portion of the still liquid bismuth must be squeezed out of the mass. That this displaced metal possesses a high degree of purity naturally follows from the fact, that the foreign ingredients being already solidified cannot accompany the protruded globules.

Perhaps the deportment here described might be taken advantage of to effect a preliminary purification of the raw metal, particularly where an opportunity exists of working on a large scale. The quantity squeezed out during one solidification amounted in weight to from 2·5 to 3 per cent. of the total quantity of the metal made use of. In the foregoing experiments I have

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not found a great divergence from this result, although with regard to the quality and quantity of the foreign substances introduced, the divergences were considerable.

The expansion of pure bismuth by solidification I have not determined. This much, however, appears to follow from the experiments here communicated, that it is far less than inaccurate experiments have hitherto induced us to estimate it.

[The interesting experiments of Dr. Schneider leave us in doubt as to whether bismuth expands at all during solidification. The following fact may perhaps throw some light upon this point. An iron bottle was completely filled with the liquid metal and closed with a screw tap; on cooling, the bismuth burst the bottle, and showed itself in a row of shining spheres along the rent. It might be urged that this was caused by the contraction of the iron, but the fact that bismuth possesses a higher coefficient of expansion than iron makes against this supposition.—Eds.]

III. *On Slaty Cleavage, as exhibited in the Devonian Limestones of Devonshire.* By Henry Clifton Sorby, *F.G.S.**

Is slaty cleavage the result of crystalline or mechanical action? This is a question I shall now attempt to answer by a description of the phænomena to be observed in the Devonian limestones of the neighbourhoods of Ilfracombe, Plymouth, and Torquay. I do not think any rocks could be found more suitable for deciding it, because they present us with cleavage in every state of development, from altogether absent to in great perfection; and also furnish us with examples of crystalline change of very varying character. A most careful examination of them, both in the field, microscopically, chemically, and by means of the polariscope, has convinced me that the structure on which their slaty cleavage depends may be completely explained on mechanical principles; and that, instead of chemical forces having given rise to it, they have had a contrary tendency; for whilst, other things being the same, the perfection of cleavage varies *directly* as the mechanical changes, it varies *inversely* as the chemical.

Before entering into the consideration of the peculiarities of structure which give rise to the cleavage, it is desirable that I should describe the actual physical and chemical constitution of the rocks themselves. To study the real nature of limestones, it is requisite to prepare sections of them that may be satisfactorily examined with high powers of the microscope, for which purpose they should be somewhere about the thousandth part of an inch

* Communicated by the Author.

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in thickness. Having prepared and examined several hundreds, including those of every geological period, I am convinced that, with the exception of concretions, and such as have undergone chemical metamorphism, nearly all may be described as organic sands or clays. In the same manner that felspar can be broken up into grains and formed into sand, or decompose into fine granules of clay, by the removal of its alkali, so can calcareous organic bodies be broken up into fragments, or decay into fine granules, by the abstraction of the organic matter that binds them together. It is thus that, in the neighbourhood of coral reefs, and in localities such as some of the West India Islands, there are deposits of both these characters, either alone or more or less mixed together, which may be most satisfactorily described as *organic sands, organic clays,* or *organic sandy clays.* Of precisely similar nature are our older British limestones, differing only in the kind and proportion of the various organisms, and in the amount of subsequent chemical change; and I shall therefore adopt this nomenclature in my descriptions of them.

The ultimate organic structure of the fragments is often so perfectly preserved in limestones, that there is no difficulty in deciding from what kind of organism they have been derived, and the mineral constituents may also be distinguished by their action on polarized light and other peculiarities. In order to know the relative bulk of the constituents, whether organic or mineral, I select a portion of the object that shows the general character in a satisfactory manner, and draw on an evenly thick piece of drawing-paper the outline of the various portions with a camera lucida, distinguishing by some mark the different constituents. I then weigh the whole in accurate balances, and afterwards cut out the various portions and weigh them. In this manner their actual proportions may be ascertained with greater precision than in most cases is necessary. Of course the difference between this kind of *physical analysis* and a chemical is very great, and very different information is afforded, as will be seen by inspecting those given further on in this paper; and it must be borne in mind that one gives the relative *volumes,* and the other the relative *weights.* Both are most valuable for particular purposes; but, for the majority of limestones, the physical method is very much more useful in geological investigations.

In most limestones, even when no chemical change has occurred, the calcareous matter derived from decayed organisms has become more or less crystalline in crystals of varying dimensions. This is often well seen in organic clays. There has also generally been introduced by infiltration a considerable amount of calcareous spar, that has filled up the cavities existing amongst

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the fragments when they were first deposited; a fact that may be seen in progress in some recent limestones. In my descriptions I shall exclude those that may be seen by the naked eye to contain much of corals or other large organisms, and only take into account the more homogeneous deposit surrounding them, constituting a limestone of which the true nature cannot be ascertained by a mere inspection of the rock in its natural state.

In the following analyses I distinguish the different specimens by the names and numbers of my microscopic objects. As an example of an organic sand of Devonian age, I select one from near Torquay.

Hope's Nose No. 1. (Physical analysis.)

Detached minute joints of encrinites, not fractured or worn	40·0
Fragments of other organisms, chiefly corals .	8·7
Organic clay, existing as dirt in the cavities of the coral, &c.	3·1
Crystallized calcareous spar, infiltered into the cavities between the organic fragments	48·2
	100·0

The fragments are from ·01 to ·05 of an inch in diameter, but chiefly from ·02 to ·03. The amount of infiltered calcareous spar agrees very well with what calculation indicates as being very probable for that of the cavities in a deposit of this character.

As an example of an organic sandy clay, I give one from near Plymouth.

Plymouth No. 3. (Physical analysis.)

Fragments of coral . . .	12·3
Portions of encrinites . .	11·5
Organic clay	25·7
Crystallized calcareous spar.	50·5
	100·0

The organic fragments are from ·01 to ·05, but on an average from ·02 to ·03 of an inch in diameter. The crystallized calcareous spar is chiefly such as was derived from the crystallization of the organic clay.

There are many limestones that contain grains of quartz and other minerals. As an illustration of this I give one from near Ilfracombe, which may be called a quartz-sandy organic clay.

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Ilfracombe No. 5. (Physical analysis.)

Quartz sand	7·6
Broken fragments of encrinites	1·6
Peroxidized pyrites	1·2
Patches of organic clay with inorganic impurities .	16·7
Crystallized calcareous spar, derived from organic clay	72·9
	100·0

The above analyses are of very characteristic limestones, but others occur whose composition is intermediate between them; and there is every gradation to pure organic clays, of which I need not give any examples, for they have no decided peculiarities except what I shall refer to presently.

I now proceed to the changes of chemical composition. Many of the limestones are more or less dolomitized, a portion of the carbonate of lime having been removed and its place occupied by carbonate of magnesia, so as to produce a mineral of a chemical composition distinctly different to that of the original deposit of organic sand or clay. In a similar manner, in some cases the carbonates of the protoxides of iron or manganese have been introduced so as to form brown-spar, which have often become peroxidized by the action of the oxygen of the atmosphere, after the rocks were elevated from their original positions, so that they now exist as higher oxides, uncombined with carbonic acid, in the form of granules in the solid calcareous crystals. I shall not now enter into a consideration of the cause of these chemical changes, but, when they have occurred, I see no reason why the rocks should not be called *metamorphic*; though, in describing them as such, it must not be thought that I confound this kind of metamorphism with such as has taken place in the vicinity of igneous rocks. Moreover, since in this change limestones have in some cases been altered in such a manner as to have plates and flakes of a different mineral composition or character developed in them, I see no reason why such should not be called *foliated*, and why this term should be restricted to the production of mica and such other minerals as are met with in rocks more commonly called metamorphic, in which chemical changes have probably been induced by the agency of heat, whereas in the others they were most likely produced by actions taking place at a temperature not much above the natural.

In some cases the difference between calcareous spar and dolomite is sufficiently well marked, but in others they cannot be distinguished by the microscope, and it is necessary to resort to chemical analysis. Those that I give are intended to illustrate physical facts, and not the mere chemical composition, and I

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therefore group the constituents accordingly, and with reference to the principles just described.

A yellowish, even-grained crystalline limestone:—

Stonehouse No. 2. (Chemical analysis.)

Carbonate of lime	54·3	true dolomite.
Carbonate of magnesia	44·7	
Carbonate of protoxide of iron (oxidized)	·8	
Peroxidized pyrites	·2	
	100·0	

In this, then, the composition is that of a true dolomite, with equal equivalents of the two carbonates and no excess of carbonate of lime.

A yellow, more marly-looking specimen than the above:—

Stonehouse No. 5. (Chemical analysis.)

Carbonate of lime	48·5	89·2 true dolomite.
Carbonate of magnesia	39·8	
Carbonate of protoxide of iron (oxidized)	·9	
Excess of carbonate of lime	10·8	
	100·0	

Here then there is 10·8 per cent. of carbonate of lime in excess. The cause of this is often well seen in sections; for they show that the rock has been originally an organic deposit, and that crystals of dolomite have been formed in some parts of it, and the rest has remained in its original condition. As an illustration of this, I select one from Paignton, whose physical constitution is very similar to *Plymouth No.* 3 given above, with the addition of crystals of dolomite and brown-spar.

Paignton No. 1. (Chemical analysis.)

Inorganic clay	1·2	
Carbonate of lime	4·0	7·3 true dolomite.
Carbonate of magnesia	2·9	
Carbonate of protoxide of iron (oxidized)	·2	
Carbonate of protoxide of manganese (oxidized)	·2	
Excess of carbonate of lime	91·5	
	100·0	

I now give one which contains a good deal of inorganic clay, and is much reddened with peroxide of iron, seen by the microscope to exist as very minute grains in the solid crystals of dolomite, and also as larger separate crystals, probably of peroxidized pyrites.

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Stonehouse No. 3. (Chemical analysis.)

Inorganic clay	22·2	
Carbonate of lime	42·2	true dolomite.
Carbonate of magnesia	32·1	
Carbonate of protoxide of iron (oxidized)	3·2	
Peroxidized pyrites	·3	
	100·0	

(Physical analysis.)

More or less detached rhombic crystals (·001 to ·005 inch in diameter)	31·2
Indistinct crystals, &c. surrounding the above	68·6
Detached red crystals of peroxidized pyrites	·2
	100·0

Near Ilfracombe occurs a limestone foliated with dolomitic crystals in a peculiar manner, as will be described further on; some parts being not yet oxidized, but others converted into yellow folia by the oxidation of the protoxides of iron and manganese.

Ilfracombe No. 8. (Chemical analysis.)

Inorganic clay	2·6	
Carbonate of lime	14·5	25·4 dolomite of equal equivalents.
Carbonate of magnesia	6·8	
Carbonate of protoxide of iron (oxidized)	3·4	
Carbonate of protoxide of manganese (oxidized)	·7	
Excess of carbonate of lime	72·0	
	100·0	

(Physical analysis of two portions.)

Rhombic crystals in bands and layers	34·5	50·8
Joints of encrinites	0·0	·8
Organic clay	65·5	48·4
	100·0	100·0

In this the amount of crystals of dolomite or brown-spar so much exceeds what would occur if the carbonates of magnesia, iron, and manganese were combined with equal equivalents of that of lime, that I conclude that the crystals must either be more highly calcareous, or part of them pure calcareous spar. This is certainly the case in a limestone from near Plymouth (*Eburton, Plymouth No.* 1), consisting of alternate folia of unaltered organic clay and dark-coloured material, which, when examined by the microscope, is seen to be composed of rhombic

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crystals with portions of peroxidized brown-spar, and on chemical analysis is found to be chiefly carbonate of lime with a little magnesia and the oxides of iron and manganese. These crystals have been developed in bands, apparently along such planes of stratification as gave lines of facility for the change to occur; and hence I should call it an organic clay, foliated along the planes of stratification with crystals of highly calcareous brown-spar. I have not yet seen any limestone, in the district under consideration, in which chemical changes have so occurred in relation to the cleavage, that it may be said to be foliated with dolomitic crystals in the true plane of cleavage, independent of stratification, in a similar manner to what has taken place with respect to some other minerals in other localities; but perhaps such might be found by a more careful examination.

These physical and chemical analyses will, I trust, suffice to show the nature of the limestones under consideration, in their unaltered condition, and when metamorphosed by subsequent chemical changes. Since corals decay more readily than such tissue as that of encrinites, I conclude that the greater part of the organic clay has been derived from them, and that the Devonian limestones were formed chiefly from the decay of corals, next to which come encrinites; whilst the proportion of other organisms is only small. The deposits have afterwards been indurated by crystallization, and the infiltration of calcareous spar; and in some cases metamorphosed by other chemical changes. Then, on elevation and exposure to the oxygen of the atmosphere, another set of changes took place, chiefly the conversion of pyrites, and the protoxides of iron and manganese, into higher oxides; a process not yet completed.

The actual constitution of the rocks being now described, I proceed to consider the phænomena of their slaty cleavage. In this communication I must forbear to enter into the facts on a large scale, seen in the field, for that alone would be a long subject. However, I must state that I am convinced that there is the most complete proof of rocks possessing cleavage having been so acted on by mechanical forces that they have been very considerably compressed in a direction perpendicular to the cleavage, and elongated to a certain extent in the line of the dip; as proved by the change in the thickness of the same bed when bent into contortions, and by various other facts described by Mr. Daniel Sharpe and myself (Quart. Journ. of Geol. Soc. vol. ii. p. 74, and vol. v. p. 111; Edinb. Phil. Journ. for 1853, vol. lv. p. 137). I shall therefore consider this to be an established fact, as seen on a large scale, and confine myself to showing that it has so altered the ultimate constitution of the rock as to produce the structure on which cleavage depends.

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If a thin section of an organic sand be examined with the microscope, it will be seen that the fragments of coral and shell are usually much longer than broad, as if derived from sections of more or less flat portions. If the rock be very thin-bedded, for instance like the Stonesfield slate, the greater part of these lie in the plane of stratification; but in such thick-bedded rocks as occur in the districts under consideration, when not cleaved, this is not the case, except with very large fragments. The smaller have their longer axes inclined in all positions, so that when a section cut perpendicular to the stratification is examined, there is seen to be no such arrangement as to give rise to any decided line of weakness, along which the rock would split in preference to any other. Let us now inquire what would be the effect on such a structure if the dimensions of the rock were changed by mechanical pressure.

If a rock has not been compressed, we may express this by saying that the ratio of the alteration in any two directions at right angles to each other is as 1 : 1; whilst if it had been compressed in such a manner that the proportion between lines of equal length before compression was changed so that in the line of pressure the length was one-sixth of that perpendicular to it, we may say that the ratio is as 1 : 6. If, for instance, before compression we had a circle, afterwards it would be an ellipse, whose axes were as 1 : 6. This is a very common amount of change in the line of dip in rocks that have a good cleavage, and so I take it by way of illustration. If, then, before compression a long-shaped fragment was inclined at any given angle to the plane perpendicular to the subsequent pressure, it may be seen from general mechanical principles, as well as proved by actual experiment, that the tangent of the angle, after the change of 1 : 6, would be one-sixth of the tangent of the original angle. Thus we have—

Angles of Inclination to the plane perpendicular to the pressure.

Originally.	After compression.
°	° ′
0	0 0
10	1 41
20	3 28
30	5 30
40	7 58
50	11 14
60	16 6
70	24 36
80	43 23
90	90 0

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According to these principles, if we suppose that in a mass of rock there were 600 particles having their longer axes lying in the space included within 5° on each side of positions inclined at 0°, 10°, 20°, &c. to the line of pressure, so that they were uniformly distributed, as is nearly the case in thick-bedded, uncleaved rocks, then, after compression so that the ratio was 1 : 6, their distribution would be changed, as shown in the following Table :—

Inclination to the direction of the pressure.	Original distribution.	Subsequent distribution.
0	600	100
10	600	103
20	600	113
30	600	134
40	600	168
50	600	236
60	600	376
70	600	733
80	600	1825
90	600	3324

It will thus be seen that the effect is to produce a great diminution in the quantity that are inclined in the direction of the pressure, and a great increase in those nearly perpendicular to it, and thus to cause a very great preponderance nearly in the plane perpendicular to the pressure. In fact, as will be seen, in a space of 10°, there, we have thirty-three times as many as in an equal one in the line of pressure; and if very small spaces were taken, since the ratio between the arc and tangent of a very small angle is one of equality, in the exact direction of the pressure, the number of particles whose axes lay in that line would be spread over six times the space, whilst perpendicular to it they would be condensed into one-sixth, and hence the relative amounts in those two positions would be as $1^2 : 6^2 = 1 : 36$. If, then, the amount of the unsymmetrical fragments was very great compared with the rest of the rock, and if their strength was such that to break them was very much more difficult than to split along them without breaking them, the resistance to fracture in the two directions would be as 1 : 36; or in other words, the facility of cleavage in the plane perpendicular to the pressure would be far greater than in one inclined at any considerable angle to it. Of course for other changes of dimensions the same results would apply. For instance, if it was 1 : 3, the relative strength would be 1 : 9, and similarly for other values.

In order to confirm these results by experiment, as mentioned

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in my paper in the Edinb. Phil. Journ. cited above*, I mixed scales of oxide of iron with pipe-clay, so as to have as uniform a structure as could be produced, and then compressed it, when I found that the arrangement was exactly such as that indicated by calculation. This will be better understood from an inspection of the accompanying figures.

Fig. 1 is a representation of a portion mixed equally, then baked and rubbed to a smooth surface; and, like a thick-bedded, uncleaved rock, it has no decided line of weakness, due to the arrangement of the particles; whilst fig. 2 is a drawing of a portion originally of similar structure, which, having been compressed, clearly shows that it has been changed, precisely in the manner I have shown by calculation to be a necessary result. The dots indicate lines along which it could easily be split, without there being any fracture of the flakes; and I think no one could fail to perceive that it would be much easier to cleave it along that direction than in any other, and that a very decided line of weakness has been produced. If then an organic clay, containing fragments of coral or shell, had its dimensions changed in a similar manner, such should be their structural arrangement, and such it really is.

Fig. 1.

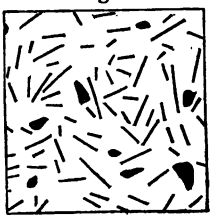

Fig. 2.

In studying the cleavage of rocks, it is best to make sections of such as have the cleavage inclined at a high angle to the stratification; for then there is no fear of confounding them together. I shall first describe the structure in the case of organic sandy clays. A very good example of this is *Plymouth No.* 3, of which the physical analysis is given above. In it the cleavage cuts the stratification at about 70°, and the fragments of coral, in place of lying in the plane of stratification, are chiefly inclined at low angles to the direction of the cleavage; in precisely the same manner as that shown to be the necessary result, if an uncleaved, thick-bedded rock of similar physical constitution had its dimensions altered by mechanical pressure. Now, in such a case as this, it is quite out of the question to refer the

* 1853, vol. lv. p. 137.

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change of structure to any crystalline action. The particles are not crystals, but are clearly proved, by their structure and form, to be fragments of organic bodies. If the cleavage was due to the mechanical cause just described, their position is most easily accounted for; but I leave the explanation of the fact, on the supposition of the instrumentality of crystalline forces, to those who advocate that view of the subject. The same is the case with the grains of quartz sand, in such limestones as contain them. For instance, in *Ilfracombe No.* 5, of which the physical analysis is given above, and in which the cleavage is perpendicular to the stratification, as seen in a section cut perpendicular to the cleavage and stratification, the grains of sand are often two or three times as long as broad, and even in some cases five times; but in place of the longer axes lying in the plane of stratification, they are chiefly inclined at low angles to the cleavage. I may here remark that there is no difficulty in distinguishing grains of sand from small crystals of quartz, such as are met with in some cherty limestones, and that there is no doubt of them being sand in this, and that their arrangement is due to the cause I have just described.

When organic clays, with or without larger fragments, have undergone consolidation, it very frequently happens that the crystallization of the granules began in various parts, and forced away the bituminous and other impurities into detached, more or less spherical spaces, which afterwards crystallized in much finer grains. On this account they now appear in sections as darker patches, giving the rock a kind of oolitic structure, though quite distinct from the more genuine oolitic grains, visible to the naked eye. These are in many cases due to crystallization taking place from certain centres, and this being therefore the reverse kind of process, I call them *positive segregational* oolites, whilst the others may be called *negative*. When the limestone has no cleavage, these negative, segregational, oolitic grains, though often of irregular form, are usually more or less equiaxed, and not very much longer in one direction than in another, and the longer axes have no particular arrangement. This is, however, very far from being the case in such as have cleavage. As examples of this I may mention *Plymouth No.* 3, *Ilfracombe No.* 5, and *Paignton No.* 1, of which the composition has been given, and in all of which the cleavage is inclined to the stratification at a high angle. In them these granules, in place of being nearly equiaxed, are so compressed and elongated that they are several times longer in the line of cleavage than in the direction perpendicular to it; and as clearly show that there has been a change in the dimensions of the rock, affecting its ultimate constitution, as do other facts, seen on a large scale, prove it with respect to the

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great mass of the beds. They indeed, on a very small scale, present us with the same phænomena as are seen in the green spots in many of the Welsh slates, as briefly described by me in the paper already referred to. In *Ilfracombe No.* 8, whose constitution is given above, which is an organic clay, foliated with lenticular portions of dolomite, it is seen that the patches of crystals have been compressed and elongated in a similar manner, and hence they lie, not exactly in the plane either of stratification or cleavage, which are inclined to one another at from 10° to 20°, but in such a direction as is the resultant of their combined influence. In a section in the plane of cleavage, the elongation in the line of dip is most clearly exhibited.

In all the cases hitherto considered the organic fragments are not very numerous, and are so imbedded in an excess of organic clay, that, when the dimensions of the rock were changed, their position was altered, but they themselves did not suffer much compression. However, it is very far from being so when there was not much organic clay, or when the compression was very great; for then they are much altered in form and structure, and sometimes to so great an extent that it was some time before I could ascertain to what kind of organism they belonged. Mr. Sharpe has shown (Quart. Journ. of Geol. Soc., vol. ii. p. 74) that the larger organic bodies have had their form much altered in cleaved rocks, and I now proceed to prove that the alteration extends in some instances to the ultimate fragments, somewhat in accordance with what he supposed to occur in all slate rocks, as described in his second communication (Quart. Journ. of Geol. Soc., vol. v. p. 111), no account being taken of change of *position*, which, according to my own observations, is a more common cause of the cleavage. For this purpose I select the minute joints of encrinites, whose form and ultimate organic structure is so very determinate and distinct. In an uncleaved limestone, such as *Hope's Nose No.* 1, of which the composition is already given, the joints of encrinites, whose diameter is on an average about $\frac{1}{10}$th of an inch, have the proportion between their breadth and length as about 3 : 2, though in some cases they are equal, or even as 4 : 5. Being thus small short cylinders, they give rise to symmetrical sections, some rectangular, and others more or less entire circles or ellipses, whose axes vary on an average from being equal to the proportion of 6 : 7, but when very elliptic 2 : 3, or even in rare cases as 3 : 5.

Of all the limestones I have examined, one from Kingskerswell near Torquay, a coral-encrinitic sandy clay, possesses the most intensely developed cleavage, so much so that it required unusual precautions in preparing the thin sections; and it is a most instructive fact, that, of all I have seen, it also shows the

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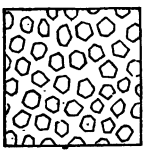
greatest amount of compression. In uncleaved limestones the joints of encrinites have their longer axes either arranged promiscuously, or if anything they lie in the plane of stratification, but in this highly cleaved specimen they are very greatly compressed in the plane of cleavage. Their forms are quite distinct from those found in uncleaved limestones; they are often not symmetrical, but broken up irregularly; and, instead of being on an average nearly equiaxed, their greatest length, which always very closely coincides with the line of cleavage, is on an average about four times that in a direction perpendicular to it; and they show forms such as would result from the compression and distortion of the sections of the short cylinders, seen in the uncleaved specimens.

Fig. 3.



But besides their form being thus changed, the ultimate organic structure is altered in a corresponding manner. When not compressed, the structure of an encrinite joint is as shown in fig. 3, drawn from a fragment in a Devonian limestone, magnified 200 linear. It is cellular, the cells being somewhat angular, varying a little in size, but on the whole nearly equiaxed. This is the character when seen in a section cut perpendicular to the axes of the joints, and in many cases it is very nearly the same in every position; but in others the cells are arranged one over the other in the line of the axes of the joints, and the walls separating them in that direction are more or less absorbed, so as to give rise to a more or less irregular or perfect tubular structure. In some cases the cells are filled with dark material, whilst their walls are clear and transparent; in others they are filled with clear, crystalline calcareous spar and the walls are dark, so that the structure may be very readily seen; but when both are equally clear and transparent or uniformly dark, it cannot be recognized.

Fig. 4.

In the highly cleaved limestone from Kingskerswell the structure of the joints of the encrinites is very different from that just described, being usually as shown in fig. 4. This has quite a different character from what is seen when the tubular structure is cut obliquely; and besides, in uncleaved limestones the tubes in the different detached joints do not lie all in one direction, but promiscuously, whereas in this limestone the longer axes of the cells lie in the line of the compression of the joints, and nearly always in the plane of cleavage, those that do not quite coincide with it, differing from it only in such a manner as would result from irregular giving way, or from the compression of a tubular structure inclined to the line of cleavage. On an average the cells have their

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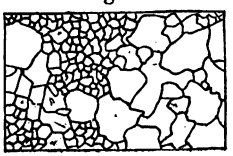
axes at least as 1 : 4, and are just of such a character as would occur from the compression of the joints to the extent which the alteration of their own form indicates. I could describe similar facts with respect to other organic bodies, but these appear to me so very clear and decisive that I think it unnecessary. In most cleaved limestones this compression of the solid organic fragments is only slight, and merely their position is altered; apparently on account of the materials by which they are surrounded having given way more readily than their own tissue; but yet it may be clearly seen that, other circumstances being the same, the amount of their compression is in proportion to the perfection of the cleavage.

Not only are the organic fragments thus compressed, but also crystals of calcareous spar and dolomite. In uncleaved limestones detached perfect rhombic crystals are often seen; and in some highly cleaved there are such forms as would result from the breaking up and compression of similar crystals. Moreover, in uncleaved limestones the calcareous crystals, filling the cavities in organic bodies or derived from the crystallization of organic clay, have their crystalline cleavage planes almost invariably straight; whilst in cleaved limestones they are often very considerably bent; and this is so particularly the case in the very highly cleaved specimen from Kingskerswell, that there is scarcely any calcareous spar that has straight cleavage planes, and much of it is so broken up and bent that it is only by comparing intermediate examples that the true nature of the structure can be ascertained. This then clearly proves that the compressing force acted so intensely and so gradually as to change the molecular arrangement even of calcareous spar and bend it, in the same manner as we may by the hand easily bend such flexible, unelastic crystals as those of talc or lead. It may perhaps be well to state that I do not think that this indicates that the rock was softened or melted by heat, but that it changed its form like a malleable substance, by the gradual movement of the ultimate atoms one over the other.

This compression and molecular rearrangment, combined with change of position, has been most effective in producing a line of weakness and cleavage in the finely crystallized organic clay of cleaved limestones. When a thick-bedded, uncleaved, fine-grained organic clay, such as the white lias of Radstock near Bath, is examined in a section cut perpendicular to the stratification, the structure is like fig. 5, which is magnified 200 linear. The stratification is in the line of the length of the figure, and, as will be seen, the longer axes of the crystals are arranged promiscuously, without relation

Fig. 5.



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to it, and are nearly equiaxed. In some thin-bedded limestones they have their longer axes in the line of stratification. The organic clay occurring in the most highly cleaved limestone of Kingskerswell has, however, a structure as shown in fig. 6, magnified 200 linear, where the crystalline granules have a very unsymmetrical character, having their axes in the plane of cleavage very much longer than perpendicular to it, as though the compression indicated by the joints of the encrinites and larger crystals had affected the smallest, constituting the ultimate structure of the rock. I think no one could compare figs. 5 and 6 without perceiving that this compression would produce a line of weakness. In fig. 5 it will be seen that there is no particular direction along which the rock could break without fracturing the crystals; whereas in fig. 6 there is clearly one along which a fracture could extend by passing amongst them, whilst in that perpendicular to it such could not be the case without fracturing very many.

Fig. 6.

Taking then all the above facts into consideration, I do not see how we can arrive at any other conclusion than that the cleaved limestones have been very considerably compressed; and that this compression has so changed the position and form of the particles of which they are composed, that such a structural weakness has been produced in a plane perpendicular to the pressure, that they may be split along it in the manner characteristic of slaty cleavage.

Having now described facts, which to my own mind carry complete conviction that slaty cleavage is the result of mechanical action, I proceed to examine what evidence there is of its being due to crystalline forces. If such were the case, there are three ways in which it might be produced. In the first place, it might be supposed that the rock was analogous to a simple, large crystal, and that the slaty cleavage was similar to crystalline cleavage planes. However, I am persuaded that no one who had examined a thin section of a cleaved rock with a microscope, so as to see what the structure really is, would for one moment advocate this view of the subject; for there is such a complete difference between them that it would in my opinion be nearly as easy to prove that stratification was the result of crystallization. If any geologist does really believe in such a cause, I can only attribute it to his supposing that cleavage planes are perfect planes; whereas the microscope shows most clearly that this is not at all the case. Even in the most perfect slates they are merely fractures passing amongst the particles, like the dotted lines in fig. 2, so as to have surfaces, smooth and level enough to the naked eye, because the grains are so minute, but seen to be

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extremely irregular when magnified sufficiently to show their real nature, and so entirely different from crystalline cleavage planes (which are straight or consist of a combination of straight lines, independent of such particles), that to compare them together and to call slaty cleavage *crystalline cleavage*, is in my opinion most inaccurate and inappropriate.

But another way in which slaty cleavage might be due to crystallization is that there were many small crystals formed, with such a crystalline polarity that there was a general line of weakness produced in the rock; and, since this view of the subject is, *à priori*, not at all improbable, I shall examine the facts to be observed in cleaved limestones, to see whether or no there is any such polarity to be found in them. The analyses given above show that various chemical changes have occurred in some of them. In many, small perfect crystals have been formed, either of calcareous spar, dolomite, or brown-spar. If then there was any tendency to crystalline polarity, we should expect that their axes would have some relation to the direction of the cleavage; but, instead of this being the case, they are inclined promiscuously in all positions. For instance, in *Stonehouse No.* 3, in a section cut perpendicular to the cleavage of the rocks in the immediate vicinity, the rhombs of dolomite are arranged in such a promiscuous manner that there is no line of weakness and no slaty cleavage. The same is the case in *Stonehouse No.* 2; and, indeed, when thus crystalline on account of chemical changes, so that the mechanical structure is obliterated, there is no cleavage whatever; and, instead of giving rise to it, we may clearly see that it has a most effective contrary action. Perhaps it may be thought that the change took place after the cleavage was produced; but this is disproved by the fact, that in contiguous specimens the dolomitic crystals have been broken up, and compressed by the mechanical actions that, according to my view of the subject, developed the cleavage. Again, in *Paignton No.* 1, the crystals of dolomite and brown-spar are seen to have no relation to the cleavage, and are neither fractured nor altered, and appear to me to have been produced afterwards. I never saw a cleaved limestone of which it might be said that the crystals were developed at the same time as the cleavage, so as to give rise to it; and I therefore conclude that the only relation between the crystals and the cleavage is that, when they were formed before it, they were effected by the mechanical compression in the same manner as organic fragments, but that they had very little, if any other influence in producing it. If they had, we should expect to find that the greater the amount of crystalline change, the more perfect would have been the development of the cleavage, whereas the very reverse is the case.

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The best method of ascertaining whether a limestone possesses any crystalline polarity is to examine a thin section of it with a polariscope, employing a magnifying power of only a few diameters. If the analyser be so arranged that no light passes through it, and if a section of a crystal of calcareous spar, cut in any direction but that perpendicular to its principal axis, be placed on a stage, so constructed that it may be rotated round the beam of light, it will be seen that in two positions at right angles to each other it has no effect on the polarized light, and the field remains dark. This is when one of its two axes of no double refraction coincides with the plane of polarization of the light; but, on rotating it from these positions, its depolarizing action gradually increases, and more and more light passes through the analyser, until the axes are inclined at 45° to it, and then its intensity gradually diminishes until the other axis coincides with it. The light, thus passing through the analyser, has the appearance of illuminating the crystal, so that it appears to become dark or light as it is rotated; being white or variously coloured according to its thickness and the inclination of the section to the principal axis. If there be a simple, single crystal, these effects take place simultaneously throughout the whole surface of the section; and, in a similar manner, if there was a number of small detached crystals, all arranged on a piece of glass with their crystalline axes in the same direction, they would on rotation all appear dark and light at the same time. If, however, they were arranged promiscuously, so that their axes were not inclined more in one line than in any other, when rotated, the general effect would remain the same in every position; whilst if there was any excess, even if small, it would easily be recognized. By placing then a section of limestone on the stage of the polariscope and rotating it, we can thus determine with very great facility whether it has any general crystalline polarity in any particular direction. In this manner it may be seen that the veins of calcareous spar, filling joint cracks in limestones, have such an excess of polarity as shows that the crystals have been formed from their sides, and that many organic fragments have a polarity related to their original form and structure; but when we examine a section of a cleaved limestone, cut perpendicular to the cleavage, containing crystals of calcareous spar, dolomite, or brown-spar, it is most clearly seen that it possesses *no crystalline polarity whatever*, or so very little as not to be recognizable even by so delicate a test. I consider this as complete a disproof as could possibly be desired of the supposition that there is any relation between their slaty cleavage and crystalline polarity.

But it may be supposed that crystalline action might cause

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layers of different mineral composition to be formed in such a manner as to produce a cleavage, without there being any crystalline polarity. Such an alternation along planes analogous to slaty cleavage, as well as that similar to stratification, does indeed occur to a certain extent in some rocks, so far metamorphosed as to have become foliated, as I described in a paper at the late meeting of the British Association at Glasgow, on the older rocks of the Scottish Highlands. However, when not thus metamorphosed and foliated, unaltered cleavage is seen by the microscope to be a fracture through homogeneous rock, independent of any such alternation of layers of different mineral composition; and therefore I conclude that this *cleavage-foliation* no more proves simple slaty cleavage to be the result of crystalline agencies, than does *stratification-foliation* indicate that they gave rise to stratification.

Summing up now the general facts contained in this paper, I show that, other circumstances being the same, the cleavage of the limestones varies directly as the amount of mechanical compression to which they have been subjected; and that the effect of this is such as would necessarily change the structure of uncleaved into that which occurs in those that are cleaved. Also, that cleaved limestones possess no crystalline polarity; and that, in place of crystallization producing slaty cleavage, it has a contrary tendency, and when perfect and complete obliterates it altogether. Though inorganic deposits do not present us with such decisive facts, yet I am persuaded that their slaty cleavage may be satisfactorily explained on similar principles, and that they agree, not only with what may be seen with high powers of the microscope, but with the structure of mountain masses. Can I therefore hesitate to conclude that *slaty cleavage is the result of mechanical and not of crystalline forces*?

IV. *An Experimental Inquiry undertaken with the view of ascertaining whether the organic actions,* Lacteal Absorption *and* Nutrition, *in the living Animal are accompanied with the manifestation of Current Force. By* H. F. Baxter, *Esq.**

THE following series of experiments, an abstract of which has already appeared in the Proceedings of the Royal Society (Nov. 25, 1852), form a continuation of an inquiry instituted for the purpose of ascertaining whether organic actions in the living animal may not be accompanied with the manifestation of electrical action. Having arrived at the conclusion in a former series†, that the organic process of *secretion* in the

* Communicated by the Author.

† Phil. Trans. 1848, 1852.

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living animal was attended with the manifestation of current force, we were naturally led to suppose that the kindred actions, such as *lacteal absorption* and *nutrition*, might also be accompanied with the manifestation of the same power: the solution of this question is the object of the present paper.

To avoid unnecessary repetition, we shall refer to our second paper* for the purpose of showing the mode in which the experiments were conducted; in that paper also will be found the precautions necessary to be observed, and some experimental arguments for the purpose of meeting certain objections that might be raised to those experiments, and as they are applicable on the present occasion we need only refer to them. We may just add, that our object is not merely to ascertain under what circumstances an effect might be produced upon the needle of the galvanometer when the electrodes are brought into contact with different parts of the living body, but to point out the *connexion* between certain *organic actions* in the living animal and the consequent *effect* upon the needle†.

§ 1. *On the manifestation of Current Force during Lacteal Absorption.*

Experiments.

Exp. 1. Cat.—Prussic acid dropped on the nose, four hours after a meal of bread and milk, having fasted fourteen hours previous.

One electrode in contact with the mucous membrane of the middle portion of the small intestine, the other in contact with the chyle flowing from the same part; the chyle *positive* 6°, and made to increase by making and breaking contact at the mercurial cups.

Another portion of the intestines was tried with similar results. The lacteals contained a milk-white fluid. We may just remark, that similar results were obtained when the mucous membrane and the blood flowing from the same part were formed into a circuit, as shown in our former series of experiments.

* Phil. Trans. 1852.

† The prejudices which exist in reference to electro-physiological pursuits are somewhat surprising. We remember being assailed with the following remark:—if you place one electrode in contact with the axilla, and the other in contact with the mouth, you may obtain an effect upon the needle, and *consequently* these results prove nothing. It may scarcely be credited that the following objections have been urged:—the *time* has not yet arrived for the prosecution of these inquiries; *physiologists* have no business to use the galvanometer; it is necessary in these researches to use a *delicate* galvanometer. We should not have noticed these objections, had they not been made in influential quarters.

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Exp. 2. Kitten.—Prussic acid dropped on the nose, two hours and a half after a meal of bread and milk, having fasted twelve hours previous.

One electrode in contact with the mucous membrane of the duodenum, the other with the chyle from the same part; the latter *positive* 4°.

The electrodes were cleaned and used in the same manner with a portion of the intestine lower down; chyle *positive* 4°; the effect made to increase by making and breaking contact.

The lacteals contained a clear and transparent fluid.

Exp. 3. Rabbit.—Prussic acid dropped on the eye, two hours after a meal of cabbage leaves.

One electrode in contact with the mucous membrane of the small intestine, the other with a supposed lacteal vessel from the same part, and afterwards with an enlarged mesenteric gland; the latter electrode *positive* in both instances 5°.

It was difficult in this instance to ascertain precisely the lacteal vessel.

Exp. 4. Rabbit.—Four hours after death; death from natural causes.

The mucous membrane of the small intestine, and what appeared to be a lacteal vessel from the same part, was wounded and formed into a circuit; mucous membrane *negative* 4°; increased by making and breaking contact.

The electrodes were cleaned, and the mucous membrane of a different portion of the small intestine and the surface of the mesentery were formed into a circuit; the mucous membrane still *negative* 4°.

Fluid blood in the veins; small intestines distended with a fluid very much like gruel; the abdominal viscera very moist.

A portion of the intestine was removed with its contents, and a circuit then formed between the inside and outside of the gut; the contents *negative* 3°.

The contents were removed by passing a stream of water through the gut; no effect occurred when the circuit was formed between the inside and outside of the intestine.

Exp. 5. Rabbit.—Twelve hours after death; death from natural causes.

Similar circuits were formed as in the last experiment, and with similar results.

Exp. 6. Rabbit.—Prussic acid dropped on the eye, two hours after a meal of cabbage leaves; the two previous meals consisted of oats.

The mucous membrane of the small intestine, and a wounded lacteal from the same part, were formed into a circuit; the latter *positive* 8°.

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Between the mucous membrane and the surface of the mesentery; the latter *positive* 5°.

Between the blood flowing from a vein (mesenteric) and the chyle from a wounded lacteal vessel; no effect.

No effect ensued when a circuit was formed between the blood and the surface of the mesentery, or between the chyle from a wounded lacteal vessel and the surface of the mesentery.

Chyle semi-opake.

Exp. 7. In this experiment a rabbit was killed in the same manner, but three hours after a similar meal, and under the same circumstances. As the results were identical, it will be unnecessary to relate them.

Exp. 8. Cat.—Prussic acid, a small quantity poured into the mouth, three hours after a meal of bread and milk.

Between the mucous membrane of the small intestine and a wounded lacteal vessel; the latter *positive* 10°. An effect occurred upon the needle on whatever part of the mesentery the latter electrode was placed, and nearly to the same amount. The lacteals contained a transparent fluid.

Exp. 9. Cat.—Prussic acid dropped on the tongue, four hours after a meal of raw meat and a very small quantity of bread and milk.

In this case the electrode in contact with the mucous membrane was *negative* 8° to that in contact with the chyle from a wounded lacteal vessel, or in contact with the mesentery: chyle transparent. We have not thought it necessary to relate the various circuits that were formed with the different organs or substances, together with the results in all these experiments, having already referred to similar circuits in our former paper.

From these experiments, we feel ourselves justified in drawing the following inferences:—

1st. That when one electrode is in contact with the mucous surface of the intestine, and the other in contact with the chyle flowing from the same part, an effect occurs upon the needle indicating the chyle to be *positive* to the contents of the intestine; and—

2nd. That this effect occurs during the organic process of *lacteal absorption.*

It may be urged that these experiments prove too much: that not only do we obtain an effect upon the needle when the electrodes are brought into contact with the mucous surface and the chyle, but also when the mucous surface and the blood are formed into a circuit, or even with the surface of the mesentery. This argument, however, cannot be adduced as *disproving* the supposition that the chyle and the intestinal contents may be in opposite electric states. We have the *positive* fact, that the needle

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indicates them to be in opposite electric states; we cannot refer these *effects* to the *combination* of the chyle with the intestinal fluid to account for the *direction* of the current, and assume that the chyle is *acid*; it may, however, be urged that the blood is positive to the intestinal secretions, as we have already shown in our former series of experiments, and that the effects are due in these experiments to the electric condition of the blood being conducted by the mesentery. We do not deny that the effects may be partly due to these circumstances; but if they were entirely due to them, how is it that no effect ensued when the chyle and the blood were formed into a circuit? would not this indicate that they are both in the same electric state?

We do not think it necessary to bring forward any physiological reasons to point out the analogy between the two organic actions, *secretion* and *lacteal absorption*, as we prefer allowing our conclusions to rest upon the *experimental* evidence we have now adduced, rather than on any *à priori* argument, except for the purpose of confirming our deductions.

§ 2. *On the manifestation of Current Force during nutrition in the Muscular and Nervous Tissues**.

The researches of Matteucci establish this important fact, that when the inner and external surfaces of a muscle in a live animal are formed into a circuit, current force is produced; and the inference which Matteucci draws from this fact is the following, that this current depends upon the vital action of nutrition. He also states, "we must never forget the analogy between the muscular electromotor element and the Voltanian element: the zinc is represented by the discs of the muscular fibre, the acid liquid by the blood, the platinum by the sarcolemma. Whatever be the conducting body with which the zinc is made to communicate with the platinum, the current is always in the same direction. . . . . The chemical actions of nutrition evolve electricity†." Reasoning from these facts and the results we had obtained in the former series of experiments, it appeared desirable to ascertain what would be the effect if we applied one electrode in con-

* The following experiments were performed previous to our knowledge of Du Bois Reymond's researches. As we do not wish to enter into the dispute which has arisen between Matteucci and Du Bois Reymond, and with our present knowledge of the researches of the latter experimentalist, we are anxious that the experiments should appear as originally presented to the Royal Society. Those who wish for further information in reference to this controversy, we must refer to the original papers of these two authors, a list of which may be found in a note appended to a paper published in the Philosophical Magazine for September 1855, entitled, "On the Force evolved during Muscular Contraction."

† Phil. Trans. 1845, p. 301.

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tact with the muscular tissue, and the other in contact with the venous blood*.

Before we proceed with our experiments we must refer to those of Du Bois Reymond. Du Bois Reymond's experiments confirm those of Matteucci, but the former appears to have been the first to show that similar effects were obtained when the *longitudinal* and *transverse* sections of a nerve were formed into a circuit, to those when the same parts were formed into a circuit with the muscular fibre.

Experiments.

Exp. 1. Kitten.—Prussic acid dropped on the nose.

The skin covering the inner part of the thigh was reflected, and the abdomen laid open; one electrode in contact with the external surface of the muscles of the thigh, the other with the blood flowing from the iliac vein; the latter *negative* 3°, but the effect did not continue: the former electrode was inserted into the substance of the muscles; no effect.

The muscles at the back part of the thigh were divided; one electrode in contact with the divided, the other with the external surface; no decisive result.

Exp. 2. Rabbit.—Prussic acid dropped on the eye.

The skin and fascia were reflected from the inner and fore part of the thigh, and the rectus femoris divided transversely; one electrode in contact with the outer, the other with the divided surface; the former *positive* 3°; by making and breaking contact made to increase to 5°.

The abdomen was laid open, one electrode in contact with the external surface of the adductor muscles of the thigh, the other with the blood from the iliac vein; an effect appeared to be produced at one time in one direction, at another time in the other.

The external and divided surfaces of the muscles at the back part of the thigh were formed into a circuit, the external slightly *positive*.

One electrode inserted into the mass on the fore part, the other into that at the back part of the thigh; no decisive effect.

One electrode inserted into the tendon of the rectus femoris, the other in contact with its external surface; no effect.

Exp. 3. Cat.—Prussic acid dropped on the tongue.

One electrode in contact with the external surface of the adductor muscles of the thigh, the other with the blood flowing from the iliac vein, the latter slightly *positive*; the former elec-

* It has always been a matter of some surprise to us that Matteucci has never performed this experiment; at least we have not been able to find any record of his having done so.

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trode was inserted into the substance of the muscle, blood still *positive.*

The external surface of the rectus and its tendon were formed into a circuit; no effect.

The external and divided surfaces of the muscles of the shoulder were formed into a circuit; a very slight effect, which soon subsided; external surface *positive.*

Exp. 4. Rabbit.—Three hours after death; death from natural causes.

The adductor muscles of the thigh were divided transversely, and the external and divided surfaces formed into a circuit; no effect. The external and afterwards the internal surface of the same mass were formed into circuits with the blood in the iliac vein; no effect.

Exp. 5. Rabbit.—Prussic acid dropped on the eye.

The skin was reflected from the back part of the leg; a vein wounded and one electrode in contact with the blood, the other with the external surface of the muscles; the latter *positive* 2°.

The muscular mass at the back part of the thigh was divided transversely, and the external and divided surfaces formed into circuit; the former slightly *positive.*

The lumbar mass of muscles was divided transversely, and a circuit formed between the external and divided surfaces; the former *positive* 3°. The effect appeared somewhat greater when the electrode was placed beneath, instead of upon, the lumbar fascia. A portion of the parietal bone was removed to expose the brain, the internal jugular vein divided at the base of the skull; one electrode in contact with the blood flowing from the vein, the other inserted into the cerebral mass; the former *positive* 8°. The electrode remaining in the brain, the other was placed in contact with the external, and afterwards with the divided surface of the lumbar mass of muscles; in both instances the latter electrode was *positive* 5°: if placed on the skin it was still *positive,* but not to the same extent.

The spinal cord was divided, one electrode inserted into its substance, the other in contact with the lumbar mass of muscles, their external and divided surfaces; these were *positive* to the former 5°.

Exp. 6. Rabbit.—Prussic acid dropped on the eye.

One electrode inserted into the brain, as in the last experiment, the other in contact with the blood flowing from the internal jugular vein; the latter *positive* 10°.

The external surface of the adductor muscles of the thigh and blood flowing from the iliac vein were formed into a circuit; the latter slightly *positive.* The muscular mass was divided and the

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electrode placed in contact with its divided surface, the other in contact with the blood; the latter *positive* 3°.

The spinal cord was divided, one electrode inserted into it, the other in contact with the divided surface of the lumbar muscles; the latter *positive* 4°.

Exp. 7. Rabbit.—Death from natural causes; twelve hours after death.

The external and divided surfaces of the lumbar muscles were formed into a circuit; the former *positive* 2°.

One electrode inserted into the spinal cord, the other in contact with the lumbar muscles; the latter *positive* 5°.

One electrode inserted into the brain, the other in contact with the lumbar muscles; the latter *positive* 5°.

The external and divided surfaces of the muscles of the thigh were formed into a circuit; external surface *positive* 2°.

Matteucci* alludes to some experiments performed by MM. Pacinotti and Puccinotti, and likewise by himself, in which the electrodes were inserted one into the brain, the other into the muscles: he says, "La déviation obtenue dans la première immersion a été toujours dans le même sens, c'est-à-dire, que le courant a été dirigé du cerveau aux muscles dans l'animal. L'intensité du courant est très-variable: j'ai obtenu quelquefois 80° et même davantage, et quelquefois 10° à 15°, et toujours dans la première immersion."

Exp. 8. Rabbit.—Prussic acid dropped on the eye.

The external surface of the muscles of the thigh and blood flowing from a vein in the groin were formed into a circuit; no effect.

Blood flowing from the internal jugular vein and the brain; blood *positive* 8°.

The external surface of the adductor muscles of the thigh, and blood flowing from the iliac vein, the latter slightly *positive*; the muscles were divided and the electrode placed in contact with the divided surface; blood *positive* 3°.

One electrode inserted into the brain, the other in contact with the external surface of the skin; the latter slightly *positive*: it was then placed in contact with the abdominal viscera, the other remaining in the brain; the former *positive* 10°.

Exp. 9. Cat.—Prussic acid dropped on the tongue.

The rectus femoris divided transversely, the external and divided surfaces were formed into a circuit; the former slightly *positive*.

The external surface of the adductor muscles of the thigh, and blood flowing from the iliac vein; the latter slightly *positive*:

* *Traité des Phénomènes Electro-Physiologiques*, p. 121.

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when the electrode was inserted into the muscular mass, blood *positive* 2°.

One electrode inserted into the brain, the other in contact with the blood flowing from the internal jugular vein; the latter *positive* 4°: the latter electrode was inserted into the muscles of the thigh, the other remaining in the brain; the former *positive* 3°. Some difficulty occurred in exposing the brain, and the aperture was small.

The external and divided surfaces of the lumbar muscles were formed into a circuit; the external *positive* 2°.

One electrode inserted into the spinal cord, and the other in contact with the lumbar muscles, and afterwards with the abdominal viscera; the latter electrode *positive* in both instances, but more so when in contact with the abdominal viscera.

Exp. 10. Cat.—Prussic acid dropped on the tongue. In removing a portion of the skull considerable hæmorrhage ensued.

One electrode in contact with the blood flowing from the internal jugular vein, the other inserted into the brain; a slight effect occurred sometimes in one direction and sometimes in the other; the electrode in contact with the brain covered with blood.

The external surface of the adductor muscles of the thigh, and blood flowing from the iliac vein; no decisive result: when the electrode was inserted into the substance of the muscles, blood slightly *positive*.

The tendon of the rectus femoris and the external, and afterwards the tendon and the divided surface, were formed into circuits; no effect.

Other circuits were formed in all these experiments, which we have not thought worth while to relate.

From these experiments we may deduce the following inferences:—1st. That when the *muscular tissue* and the *venous* blood from the same limb are formed into a circuit, the effect upon the needle indicates the blood to be *positive*, but slightly so; and 2nd, that when the *nervous* tissue and the *venous* blood are formed into a circuit, the blood is *positive**.

Other inferences, confirming the experiments of Matteucci and other Italian philosophers, may be also drawn. But do not the facts we have related confirm the inference deduced by Matteucci as to the *origin* of the *muscular* current? And may we not draw the same inference respecting the *nervous* current, viz. that *during the process of nutrition in the living animal the tissues (the muscular and the nervous) and the venous blood are in opposite electric states?* If the experimental evidence, however, be considered as not affording such direct evidence as in the case of

* We do not attempt in these experiments to inquire into the *force* of the current, but merely to ascertain its *existence*.

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secretion, we may nevertheless adduce physiological reasons, viz. the analogy which exists between the two processes—between *secretion* on the one hand and *nutrition* on the other,—in support of our conclusions.

Concluding Remarks.

As these inquiries have met with a degree of opposition which we can only refer to the strong prejudices which exist in reference to electro-physiological pursuits, since no attempt that we are aware of has been made to refute either the conclusions or the experiments by *experimental* evidence, we nevertheless feel compelled to reply to one or two objections which have been raised, more especially in reference to our latter experiments. It has been stated that our experiments do not *confirm* those of Du Bois Reymond, and that it is necessary in these researches to use a *delicate* galvanometer. Let us first notice the former objection, as it will be a means of refuting the latter.

These experiments, as we have already stated, were undertaken previous to the knowledge of Du Bois Reymond's researches. The facts which Du Bois Reymond has elicited, and which we wish to draw attention to, are those in reference to the law of the muscular current and to that of the nervous current; the former confirming the experiments of Matteucci, the latter being those which Du Bois Reymond appears to have been the first to elucidate. Our experiments, as far as we can see, not only confirm those of Matteucci, but also tend to confirm those of Du Bois Reymond. Our object, however, was to ascertain the *origin* of the current (*muscular* current) in Matteucci's experiment; and the question is, Do not our experiments tend also to point out the *origin* of the *nervous* current in Du Bois Reymond's experiment? And surely we may allude to one circumstance, which ought to be gratifying rather than a subject of dispute, namely, that three inquirers, working independently of each other, should ultimately arrive at results which tend to confirm and support the conclusions of each independent observer. Moreover, may we not adduce this circumstance also as an argument in favour of the conclusions?

In looking over our experiments, however, we find that we have not succeeded in obtaining an effect upon the needle when the tendon and the surface of the rectus femoris muscle were formed into a circuit. Du Bois Reymond appears to consider this experiment of some importance; we, on the other hand, do not consider it in the same light. We do not deny that it may be obtained*. The question, however, is this: Has Du Bois Rey-

* We are assuming that this is the point of objection raised; we have never been able to get any definite and tangible objection explicitly stated.

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mond himself always been able to obtain it? Has he not been compelled to suppose that there exists a layer, which he calls the *para-electronomic* layer, beneath the tendon capable of counteracting the current, which, when it does occur, soon subsides? What evidence have we of the existence of this layer beyond that of its enabling us to account for the non-appearance of the assumed current? And here, in conclusion, we cannot refrain from quoting some remarks we formerly made, and which appear to us to be applicable on the present occasion. In alluding to the possibility of effects occurring with different galvanometers, we stated, "We do not deny, but think it highly probable, that with *delicate* galvanometers some effect might occur. Assuming that a slight effect were obtained, it would then become a question whether the effects were not due to the changes which occur at the electrodes, rather than at the points of nutrition or secretion. The physical philosopher has an undoubted right to call upon the physiologist to point out the *anion* and *cation* in his circuit, or some adequate *cause* for the *current*. The fact is, the vagueness associated with the term *current* has misled physiologists. *We are firmly convinced, that, without extreme care, a delicate galvanometer would only lead to confusion; there is no difficulty in obtaining an effect upon the needle; if anything, we obtain more than we want: the great point is to account for it when obtained,* i. e. *to show with what class of phænomena the effects may be referred**." We may just add, that these remarks were not made in reference to any particular experiments, or that we intended to decry the use of *delicate* galvanometers, but to show that, in the employment of *delicate* instruments, greater caution would be requisite in deducing our conclusions.

[With reference to the foregoing paper, we would express the opinion that the subject treated of is not to be advanced by experiments executed in the manner described. The results appear to us to belong to a class which could be obtained without the animal body as well as within it; they may add to our knowledge of general electromotive actions, but not to our knowledge of animal electricity.—Eds.]

V. *On the Solar and Lunar Diurnal Tides of the Coasts of Ireland. By the* Rev. Samuel Haughton, *Fellow of Trinity College, Dublin*†.

IN the autumn of 1850, tidal observations were commenced at twelve stations on the coasts of Ireland, under the direction of the Committee of Science of the Royal Irish Academy.

* Phil. Trans. 1852, p. 286.

† Results of a paper read before the Royal Irish Academy, April 24, 1854. Communicated by the Author.

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One of these stations, Kilrush, Co. Clare, was abandoned shortly after the commencement of the observations, in consequence of difficulties experienced in obtaining a sufficiently sheltered position for the tide-gauge; and at another station, Killibegs, Co. Donegal, the observations made were not of so complete a character as at the remaining ten stations.

At the request of the Committee of Science of the Royal Irish Academy, I undertook the task of reducing and discussing the tidal observations, the reduction of the meteorological observations being undertaken by the Rev. Humphrey Lloyd, whose 'Notes on the Meteorology of Ireland,' deduced from those observations, have been recently published by the Academy.

The tidal observations made under the direction of the Academy were of two distinct kinds: the first being the observation of all the high and low waters at each of ten stations for periods varying from sixteen to twelve months; the second being the observation of complete tides at intervals of fifteen minutes, the tides selected for this purpose being four in each lunation, two spring and two neap; these observations were made at eleven stations, and, like the former, extend over a period varying from sixteen to twelve months.

These two classes of observations were made for the purpose of throwing light upon different questions connected with the laws of the tides; the first class of observations being intended to furnish data for the separation of the effects of the sun and moon in the diurnal tide, a problem not hitherto solved by observation; and the second class of observations being intended to illustrate the laws of the semidiurnal tide, particularly in the Irish Channel, and to decide the true mean height of the water round the coasts of Ireland.

In the present communication, I shall give the results of the calculations made from the daily observations, with a view to determine the separate effects of the sun and moon upon the diurnal tide.

Section I. *Description of the Tidal Stations and of the Tide-gauge used in the observations.*

I. Castletownsend, Co. Cork.

Lat. 51° 31′ N. Long. 9° 7′ W.

The zero of the tide-gauge was carefully referred to the iron bolt driven vertically into the rock in which the Coast-guard signal-staff is secured.

The zero was 31·91 feet below this bolt. The gauge at this station was placed in the open sea, and was held in its place by stays and guys made fast to the rock.

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II. Caherciveen, Co. Kerry.

Lat. 51° 57′ N. Long. 10° 8′ W.

The zero of the tide-gauge, which was erected in the N.E. angle above the bridge, was referred to a provisional bench-mark made on the corner coping-stone of the bridge. The zero was 23·51 feet below this mark.

III. Kilrush, Co. Clare.

Lat. 52° 38′ N. Long. 9° 26′ W.

The tide-gauge was placed at this station on the sea-face of the steam-boat pier, and consequently exposed to the gales from the S.W. This was the only position in which it could be placed, and unfortunately it was twice washed away by the violence of the waves.

The zero was referred to the copper bolt driven vertically into one of the facing-stones of the pier, and was found to be 20·59 feet below this bolt.

IV. Bunown, Co. Galway.

Lat. 53° 24′ N. Long. 10° 2′ W.

The tide-gauge at this station was erected at the inner side of the new pier built for the accommodation of fishing-boats, and was well sheltered from the west and south-west.

V. Killibegs, Co. Donegal.

Lat. 54° 38′ N. Long. 8° 24′ W.

Owing to the impossibility of erecting a tide-gauge at this station in a position which would not be left dry at low water, it was determined to dispense with the daily observations, and to make the weekly observations with two tide poles, one of which was fixed to the pier near the Coast-guard house, and the other on a rock at a short distance from the shore, the latter being used only when the base of the pier was dry at low water of spring tides. The correspondence of the figures on the two poles was carefully verified.

The zero of the tide-pole was 18·00 feet below the coping-stone of the pier to which it was fastened.

VI. Rathmullan, Co. Donegal.

Lat. 55° 7′ N. Long. 7° 32′ W.

The tide-gauge was erected in a sheltered situation at the inner side of the pier.

Its zero was 20·20 feet below the upper surface of the corner coping-stone at the southern end of the pier.

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VII. Portrush, Co. Antrim.

Lat. 55° 12′ N. Long. 6° 38′ W.

The tide-gauge was erected in an angle of the northern pier, close to the spot in which the tidal observations were made in 1842. It was referred to the copper bolt driven vertically into one of the facing-stones of the quay, and its zero was found to be 12·58 feet below this bolt.

VIII. Cushendall, Co. Antrim.

Lat. 55° 4′ N. Long. 6° 4′ W.

The tide-gauge at this station was erected on the landward side of the new pier in Red Bay.

The zero of the gauge was referred to the Ordnance bench-mark on the top of the wall, at the road side, north of the tunnel, above the pier; it was found to be 34·74 feet below this mark.

IX. Donaghadee, Co. Down.

Lat. 54° 38′ N. Long. 5° 33′ W.

The tide-gauge was erected beside the pier, close to the copper bolt driven vertically into one of the facing-stones of the quay, in a sheltered position, and with deep water at the lowest tides.

The zero of the gauge was 19·80 feet below this bolt.

X. Kingstown, Co. Dublin.

Lat. 53° 17′ N. Long. 6° 8′ W.

The gauge was placed in the inner angle of the new harbour, and was well sheltered from all points, particularly the north-east, from which direction large waves often enter Kingstown harbour.

Its zero was referred to the copper bolt in the coping-stone of the pier near the water-tank, and found to be 18·28 feet below this bolt.

XI. Courtown, Co. Wexford.

Lat. 52° 40′ N. Long. 6° 12′ W.

Some difficulty was found at this station in selecting a suitable position for the tide-gauge, in consequence of the harbour having become partially filled with sand and gravel forced into it by the sea. The gauge was placed beside the wooden landing-stage in the open sea, in rather an exposed position.

Its zero was found to be 17·13 feet below the copper bolt driven vertically into one of the facing-stones of the entrance to the harbour.

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XII. Dunmore East, Co. Waterford.

Lat. 52° 8′ N. Long. 6° 57′ W.

The tide-gauge was erected at the inner angle of the harbour in a very sheltered position.

Its zero was referred to the copper bolt driven vertically into one of the facing-stones of the pier, not far from the light-house. It was found to be 17·59 feet below this bolt.

The twelve tidal stations just described were established between September 1850 and January 1851, and were each visited twice during the observations.

The time was found at each station by means of a vertical gnomon, with a meridian line, the observation of which at mid-day, with the aid of a table of the equation of time furnished to each observer, gave the local time with considerable accuracy.

The tide-gauge consisted of a wooden case, from 20 to 28 feet in length, placed in a vertical position and closed at the bottom, excepting a few holes, guarded by copper gauze. The bottom of the case was placed 4 or 5 feet below low-water mark, and the oscillations of the water outside were scarcely sensible within the case. To the top of the case was attached a box containing a drum, over which was passed a silk cord, terminating at one extremity in a wooden float resting on the water, and at the other extremity in a small leaden counterpoise. The motion of the water inside the case was communicated by this cord to the drum, which was connected by wheel-work of a very simple character with the index-hand of a dial, marked into sixteen feet, each divided into tenths.

These dials and the annexed wheelwork were made by Mr. Dobbin, of Wicklow Street, Dublin, and worked remarkably well during the whole time of being used. In addition to the index-hand traversing the dial, two other hands were placed on a separate axle, which were pushed in opposite directions by a projection placed on the index-hand, thus registering without observation the maximum and minimum heights of the tide. To obtain this registry it was only necessary to visit the dial twice during each lunar day, either at half-flood or half-ebb; and after a few days' practice, no difficulty was experienced by the observers in recording all the high and low waters, with a very slight expenditure of time.

The greatest care was taken to secure accurate determinations of the exact position of the zero marked outside the case, with reference to the Ordnance and other bench-marks, the zero of the dial being made to correspond with the zero outside. The gnomons by which the time was observed were also erected with

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care; and I believe that, with good observers, the error in time would be less than one minute. To the observers themselves, who were all selected from the Coast-guards at each station, too much praise can scarcely be given for the intelligence and patient industry with which they succeeded in carrying out the rules for observation in which they were carefully instructed; and I believe it is not too much to assert, that, so far as the observers were concerned, it would be impossible to have an extensive series of tidal observations made with greater care and accuracy.

SECTION II. *Method of discussing the Daily Observations.*

The daily observations consisted, as already mentioned, of observations of all the apparent high and low waters occurring each day. These observations of height were arranged in order of occurrence, and the diurnal tide in height at high and low water calculated from them, in the following way.

The apparent height of the tide at any moment is made up of several quantities, of which the principal are,—

1. The semidiurnal tide.
2. The diurnal tide.
3. Tides of long period depending on the change of position of the sun and moon, or the semimenstrual and semiannual tides.
4. Elevation or depression of the water due to slow changes of barometric pressure.
5. Abrupt changes due to wind.

It is possible, by the following method, to separate in the observed high and low waters, the part due to diurnal tide and abrupt changes due to wind, from the 1st, 3rd, and 4th quantities just mentioned.

Let h_1, h_2, h_3, h_4, h_5 be five successive high or low waters; the parts of these heights due to the first four causes can be represented by sines and cosines. Let $A \cos n\phi$ be the height due to any periodic cause, ϕ being an arc of fixed magnitude, and n a quantity increasing with the time in such a way that it is increased by unity in the interval between two high or low waters, *i. e.* in about $12^h\ 24^m$.

From this definition we have—

$$h_1 = A \cos (n-2)\phi$$
$$h_2 = A \cos (n-1)\phi$$
$$h_3 = A \cos n\phi$$
$$h_4 = A \cos (n+1)\phi$$
$$h_5 = A \cos (n+2)\phi.$$

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Taking the fourth difference, we have, after some transformations,

$$4\text{th diff.} = h_1 - 4h_2 + 6h_3 - 4h_4 + h_5 = 16A \cos n\phi \sin^4 \tfrac{1}{2}\phi.$$

The right-hand member of this equation disappears for all the terms except the diurnal tide. For, in the semidiurnal tide, the value of ϕ is nearly 360°, and consequently $\sin^4 \frac{1}{2}\phi$ is evanescent; for the tides of long period, such as those under the third and fourth heads, ϕ is a very small angle; for example, in the semimenstrual tide, ϕ is about 12° 37′, and therefore

$$\frac{\text{4th difference}}{16A \cos n(12° 37')} = \sin^4(6° 18') = 0{\cdot}000145,$$

a quantity which is perfectly insensible.

The slow changes of level due to the slow changes of atmospheric pressure will in like manner disappear from the 4th difference of the heights at high and low water, and there remains therefore nothing to consider but the diurnal tide, and the accidental changes due to sudden variations of wind; the latter cannot be eliminated by any process of calculation, as they simply produce the effect of making a particular height, or two or three successive heights, differ from their true values; they are to be considered as in the same category as errors of observation; and so far as they occur, they vitiate the observations which they affect. In the diurnal tide, on the contrary, the value of ϕ is nearly 180°, and therefore $\sin^4 \frac{1}{2}\phi$ is nearly unity; and therefore the whole effect of the diurnal tide remains in the 4th difference of the successive heights, or

$$\text{Diurnal tide} = \frac{h_1 - 4h_2 + 6h_3 - 4h_4 + h_5}{16} \quad (1)$$

Having arranged the high and low waters for the ten stations in regular order, I employed two calculators, who were unacquainted with each other's name and address, to calculate the diurnal tide for the high and low waters following the moon's southing, from equation (1). I then compared these independent calculations, and whenever they differed, I repeated the calculation myself, and in this manner secured the perfect accuracy of the Tables, from which the results of this paper are calculated. Notwithstanding the accuracy of observation obtained by the form of tide-gauge used by us, and the evident care of the observers, there are occasional irregularities in these figures which must be attributed to the fifth cause mentioned in p. 52. And such irregularities occur principally during the stormy part of the year, and occasionally on the occurrence of isolated storms; but, on the whole, I believe the present observations of the

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diurnal tide are the most perfect that have been ever made on so large a scale and for such a length of time.

Having thus eliminated the diurnal tide from the observed heights, I constructed the diurnal tide at high and low water following the moon's southing, by points, on paper ruled into divisions of tenths of an inch; on the scale of heights, of an inch to the foot; and of time, of five lunar days to the inch. After joining the points, a curve was drawn in the usual way, which represented geometrically the actual results of observation. These curves were then compared with other curves constructed from theory in the following manner.

From whatever theory of tides we set out, whether Equilibrium theory, Laplace's dynamical theory, or Mr. Airy's theory of canal waves, we arrive at the result that the diurnal tide is proportional to the product of the sine and cosine of the declination of the luminary; and the most general form of diurnal tide may be deduced from this supposition, combined with the well-known fact that the tide does not accompany, but follows the southing of the luminary; and with the hypothesis of the hydrodynamical theories, that the position of the luminary corresponding to any tide is not its actual position, but the position it had at a period preceding the period of the tide, by an interval called the age of the tide. We may therefore consider the following expression as the most general expression for the height of the diurnal tide; at least it is the expression deduced from theory with which I have compared the observed diurnal tide,

$$D = S \sin 2\overline{\sigma} \cos (s - i_s) + M \sin 2\overline{\mu} \cos (m - i_m). \quad (2)$$

In this equation,—

D is the height of the diurnal tide at the high or low water following the moon's southing, expressed in feet.

S and M are the coefficients in feet of the solar and lunar diurnal tides.

$\overline{\sigma}$ and μ are the declinations of the sun and moon, at a period preceding the high and low water, by an interval to be determined for each luminary, and called the age of the solar and lunar diurnal tide.

s and m are the hour-angles of the sun and moon west of the meridian at the time of high or low water.

i_s and i_m are the diurnal solitidal and lunitidal intervals, or the time which elapses between the sun's or moon's southing and the solar or lunar diurnal high water.

The right-hand member of equation (2) therefore contains eight quantities, of which two only, m and s, are known directly by the observed time of apparent high and low water; the

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remaining six, three belonging to the solar, and three to the lunar diurnal tide, are to be determined, and being found, the values of D calculated from (2) are to be compared with its values deduced from observation in the way already described.

The unknown quantities of the diurnal tide are therefore,—

1st. The coefficients of solar and lunar tides.

2nd. The diurnal solitidal, and lunitidal intervals.

3rd. The ages of the solar and lunar tides.

Of these quantities, one, viz. the age of the solar diurnal tide, cannot be found from observation, because the sun's place or declination changes so slowly that it is a matter of indifference what place we assign to the sun (within a limit of some days) in estimating the amount of the solar tide. The other five quantities may and have been found from the observations, as I shall presently show.

The constants of equation (2) were found as follows for each of the ten tidal stations. An inspection of equation (2) shows that the solar diurnal tide disappears at the equinoxes (because $\bar{\sigma} = 0$, or is very small), hence the equinoctial diurnal tide observed at high and low water is altogether due to the moon. The lunar diurnal tide was thus found approximately from the equinoctial tides, and was constructed on the same abscissæ as the observed diurnal tide. This lunar tide, constructed from calculation, differs considerably from the observed diurnal tide at the solstices, the difference being due to the solar diurnal tide. In this way the solar diurnal tide was in its turn calculated approximately from the solstitial tides, and the calculated solar tide carefully superposed upon the lunar tide.

The observed and calculated tides, constructed as just described, were then compared, both with reference to the maximum heights at high and low water, both positive and negative; and with reference to the times of vanishing of the diurnal tide at high and low water; and from this comparison the constants used in the construction were corrected, and the heights and times again compared, until the agreement was as close as the observations would allow.

The constants thus successively corrected are those given for each locality, and the comparison of the observed and calculated tides is also given, so as to afford a very good idea of the degree of agreement between the observations and theory.

The unknown constants of equation (2) are—

Lunar Diurnal Tide.

1. Age of tide.
2. i_m = lunitidal interval.
3. M = coefficient of lunar tide.

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4. Age of tide.
5. i_s = solitidal interval.
6. S = coefficient of solar tide.

These constants were found as follows from the comparison of the observed and calculated tides:—

1. The age of lunar tide was found from the comparison of the times of vanishing of the observed and calculated tides.

2. The lunitidal interval $= i_m$ was found from the equation

$$\cot(m - i_m) = \frac{\text{Range of lunar diurnal tide at high water}}{\text{Range of lunar diurnal tide at low water}} \quad (3)$$

4. The lunar coefficient = M was found from the equation

$$2M \sin 2(\text{max. value of } \mu) = \sqrt{\begin{array}{l}(\text{Range of lunar diurnal tide at high water})^2 \\ +(\text{Range of lunar diurnal tide at low water})^2\end{array}} \quad (4)$$

4. The age of solar tide was not determined.

5. The solitidal interval $= i_s$ was found from the comparison of the solstitial intersections of the observed diurnal tide with the calculated lunar tide.

6. The coefficient of the solar tide = S was found from the equation

$$2S \sin 2(\text{max. value of } \sigma) = \text{maximum range of solar diurnal tide} \quad (5)$$

SECTION III. *Diurnal Tide at Castletownsend.*

Having constructed the observations contained in the calculated tables by means of curves, as already described, I found it impossible to separate the effects of the sun and moon. The tide is so small, and its times of vanishing consequently so badly marked, that it was not possible to divide it with any kind of certainty into a solar and lunar tide. I therefore supposed the tide to be due to the moon only, and made the following inferences, which I do not, however, consider as of high value.

The mean of all the maximum values of the tide at high water was found to be +0·0885 and −0·0835, giving an average range at high water of 0·1720 ft.

The mean of all the maximum values at low water was found to be +0·0820 and −0·0906, giving an average range at low water of 0·1726 ft.

If, therefore, h and l represent the ranges of diurnal tide at high and low water respectively, we have, by equation (2),

$$h = 2M \sin(2 \text{ max. declination}) \cos(m - i_m),$$

$$l = 2M \sin(2 \text{ max. declination}) \cos(90° + m - i_m);$$

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and consequently, by equation (3),

$$\frac{h}{l} = -\cot(m - i_m).$$

Substituting for h and l their values, we find

$$\frac{0{\cdot}1720}{0{\cdot}1726} = \cot(m - i_m) = \cot(45^\circ\ 6');$$

and, converting 45° 6′ into time, we have

$$m - i_m = 3^h\ 6^m;$$

but m, which is the establishment at Castletownsend expressed in local time, is equal to $4^h\ 17^m$, and therefore

$$i_m = 1^h\ 11^m.$$

Equation (4) also gives us the relation

$$2M \sin(2 \text{ max. declination}) = \sqrt{h^2 + l^2}.$$

Hence

$$2M = \frac{\sqrt{h^2 + l^2}}{\sin(42^\circ)} = \frac{0{\cdot}234}{0{\cdot}669} = 0{\cdot}363 \text{ feet.}$$

If, therefore, the diurnal tide at Castletownsend be supposed wholly due to the moon, it may be expressed by the formula

$$D = 0{\cdot}181 \sin 2\mu \cos(m - 1^h\ 11^m).$$

In this equation, μ, the moon's declination, is to be assumed for a period preceding the time of observation. The length of this period or age of the tide could not be ascertained in consequence of the irregularity of the times of vanishing. It appears from the preceding investigation, that the maximum effect in raising or lowering the sea produced by the diurnal tide at Castletownsend is

$$0{\cdot}181 \text{ ft.} \times \sin 42^\circ = 0{\cdot}117 \text{ ft.} = 1{\cdot}4 \text{ inch},$$

the total effect both ways being less than 3 inches. It is not surprising that it should be difficult to separate such a small effect as this into a solar and lunar tide.

SECTION IV. *Diurnal Tide at Caherciveen.*

In discussing the solar and lunar diurnal tides involved in the tables calculated for Caherciveen, the following results were arrived at:—

I. *Diurnal tide in height at high water.*

1. Maximum value of lunar tide for positive heights = 0·15 ft.
2. Maximum value of lunar tide for negative heights = 0·20 ft.
3. Maximum value of solar tide = 0·245 ft.
4. Diurnal solitidal interval = $3^h\ 28^m$.
5. Age of lunar tide = $5^d\ 4^h$.

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II. *Diurnal tide in height at low water.*

1. Maximum value of lunar tide for positive heights = 0·230 ft.
2. Maximum value of lunar tide for negative heights = 0·300 ft.
3. Maximum value of solar tide = 0·245 ft.
4. Diurnal solitidal interval = $3^h\ 28^m$.
5. Age of lunar tide = $4^d\ 17^h$.

Adding together the first two of each of the preceding series of values, we find,—

Range of lunar tide at high water $= h = 0{\cdot}350$ ft.
Range of lunar tide at low water $= l = 0{\cdot}580$ ft.

Hence by equation (3),

$$\cot(m - i_m) = \frac{0{\cdot}350}{0{\cdot}530} = \cot(56^\circ\ 34'),$$

which, converted into time, gives

$$m - i_m = 3^h\ 54^m;$$

but m, the moon's hour-angle at high water in Caherciveen time, is $3^h\ 48^m$, and therefore

$$i_m = 0^h\ 6^m.$$

By equation (4), we have

$$\text{max. value of } 2M \sin 2\mu = \sqrt{(0{\cdot}35)^2 + (0{\cdot}53)^2} = 0{\cdot}635 \text{ ft.},$$

from which we obtain

$$M = 0{\cdot}480 \text{ ft.}$$

And since the maximum value of the solar tide at high water is 0·245 feet, we have, by equation (5),

$$\text{max. value of } 2S \sin 2\sigma = 0{\cdot}490 \text{ ft.};$$

therefore

$$S = 0{\cdot}335 \text{ ft.}$$

Combining together the preceding results, we have the following tidal constants for Caherciveen:—

1. Lunitidal interval $= 0^h\ 6^m$.
2. Solitidal interval $= 3^h\ 28^m$.
3. Age of lunar tide $= 5^d\ 4^h$ at high water.
   do. do. $= 4^d\ 17^h$ at low water.
4. $M = 0{\cdot}480$ ft.
5. $S = 0{\cdot}335$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M} = 0{\cdot}698$.

The solar and lunar tides were constructed from the preceding constants, and compared with the observed tides. The results of this comparison are contained in the following Tables.

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Caherciveen Tide, Table A.

Positive heights at high water for sixteen lunations, commencing 1850, October $20^d\ 17^h\ 30^m$, and ending 1852, January $1^d\ 11^h\ 54^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·14	0·16	−0·02	9	0·20	0·19	+0·01
2	0·27	0·20	+0·07	10	0·23	0·30	−0·07
3	0·23	0·24	−0·01	11	0·24	0·29	−0·05
4	0·24	0·29	−0·05	12	0·20	0·26	−0·05
5	0·28	0·27	+0·01	13	0·13	0·19	−0·06
6	0·22	0·21	+0·01	14	0·11	0·09	+0·02
7	0·08	0·08	0·00	15	0·18	0·10	+0·08
8	0·14	0·13	+0·01	16	0·28	0·21	+0·07

Mean difference = +0·004 ft.

Caherciveen Tide, Table B.

Negative heights at high water for sixteen and a half lunations, commencing 1850, October $7^d\ 21^h\ 6^m$, and ending 1852, January $1^d\ 11^h\ 54^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·23	0·22	+0·01	10	0·34	0·34	0·00
2	0·24	0·22	+0·02	11	0·38	0·37	+0·01
3	0·35	0·24	+0·11	12	0·36	0·35	+0·01
4	0·37	0·37	0·00	13	0·27	0·33	−0·06
5	0·28	0·35	−0·07	14	0·25	0·16	+0·09
6	0·26	0·31	−0·05	15	0·14	0·12	+0·02
7	0·17	0·27	−0·10	16	0·28	0·25	+0·03
8	0·14	0·15	−0·01	17	0·31	0·34	−0·03
9	0·19	0·17	+0·02				

Mean difference = 0·000 ft.

The following Tables show the comparison of the observed and calculated diurnal tide at low water at Caherciveen.

Caherciveen Tide, Table C.

Positive heights at low water for sixteen and a half lunations, commencing 1850, October $9^d\ 2^h\ 3^m$, and ending 1852, January $5^d\ 15^h\ 6^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·27	0·30	−0·03	10	0·41	0·45	−0·04
2	0·40	0·37	+0·03	11	0·40	0·40	0·00
3	0·44	0·43	+0·01	12	0·32	0·31	+0·01
4	0·41	0·44	−0·03	13	0·30	0·27	+0·03
5	0·35	0·37	−0·02	14	0·28	0·28	0·00
6	0·27	0·24	+0·03	15	0·32	0·38	−0·06
7	0·28	0·27	+0·01	16	0·40	0·43	−0·03
8	0·40	0·35	+0·05	17	0·46	0·46	0·00
9	0·42	0·41	+0·01				

Mean difference = −0·002 ft.

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Caherciveen Tide, Table D.

Negative heights at low water for sixteen lunations, commencing 1850, October $24^d\ 0^h\ 0^m$, and ending 1851, December $21^d\ 15^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·36	0·40	−0·04	9	0·52	0·56	−0·04
2	0·55	0·59	−0·04	10	0·63	0·60	+0·03
3	0·51	0·55	−0·04	11	0·45	0·46	−0·01
4	0·37	0·46	−0·09	12	0·40	0·38	+0·02
5	0·43	0·38	+0·05	13	0·32	0·32	0·00
6	0·36	0·31	+0·05	14	0·41	0·37	+0·04
7	0·38	0·36	+0·02	15	0·50	0·50	0·00
8	0·49	0·49	0·00	16	0·55	0·52	+0·03
Mean difference = +0·001 ft.							

The four preceding Tables show the agreement in height between the observed and calculated tides at Caherciveen. The following Tables show the differences of the observed and calculated times of vanishing of the diurnal tide, during the sixteen and a half lunations of observation.

Caherciveen Tide, Table E.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide = $5^d\ 4^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	0·00	12	0·00	23	0·00
2	−1·75	13	0·00	24	−0·50
3	0·00	14	0·00	25	+0·85
4	0·00	15	+2·40	26	−1·25
5	+2·00	16	−3·25	27	+1·50
6	−0·75	17	0·00	28	0·00
7	+0·25	18	−1·50	29	0·00
8	+0·90	19	−1·50	30	+0·50
9	+0·75	20	0·00	31	−1·00
10	0·00	21	−0·35	32	+1·00
11	+1·75	22	0·00		
Mean difference = −0·002 lunar days.					

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Caherciveen Tide, Table F.

Difference of observed and calculated times of vanishing at low water, expressed in lunar days.

Age of lunar tide = $4^d\ 17^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·45	12	−0·95	23	−0·55
2	−0·55	13	−1·80	24	−0·40
3	−0·15	14	+0·45	25	−0·15
4	+0·45	15	+0·05	26	+0·95
5	+0·45	16	+0·45	27	+1·45
6	+0·70	17	+0·45	28	+0·80
7	+0·45	18	+0·45	29	+0·80
8	+1·40	19	0·00	30	+0·45
9	+0·45	20	−1·15	31	+0·45
10	−2·05	21	−1·40	32	−1·05
11	−0·15	22	−0·40	33	+0·90

Mean difference = −0·003 lunar days.

The agreement between the observed and calculated heights and times shown in the preceding Tables, is as close as can be expected in tidal observations, and indicates the degree of importance which should be attached to the diurnal tidal constants at Caherciveen.

SECTION V. *Diurnal Tide at Bunown.*

Separating the solar and lunar tides in the diurnal tide at Bunown, deduced from the observed heights by the method already described, I obtained the following results:—

I. *Diurnal tide in height at high water.*

1. Maximum value of lunar tide for positive heights = 0·20 ft.
2. Maximum value of lunar tide for negative heights = 0·28 ft.
3. Maximum value of solar tide = 0·25 ft.
4. Diurnal solitidal interval = $2^h\ 52^m$.
5. Age of lunar tide = $4^d\ 9^h$.

II. *Diurnal tide in height at low water.*

1. Maximum value of lunar tide for positive heights = 0·30 ft.
2. Maximum value of lunar tide for negative heights = 0·40 ft.
3. Maximum value of solar tide = 0·25 ft.
4. Diurnal solitidal interval = $2^h\ 52^m$.
5. Age of lunar tide = $4^d\ 9^h$.

Adding the first two of each of the preceding, we find—

Range of lunar tide at high water = 0·48 ft.
Range of lunar tide at low water = 0·70 ft.

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Hence by equation (3),

$$\cot(m-i_m) = \frac{0{\cdot}48}{0{\cdot}70} = \cot(55^\circ),$$

or

$$m-i_m = 3^h\ 47^m;$$

but since m, at high water, is the establishment expressed in Bunown time, and is $4^h\ 18^m$, we find

$$i_m = 0^h\ 31^m.$$

By equation (4), we have

$$\text{max. value of } 2M \sin \overline{2\mu} = \sqrt{(0{\cdot}48)^2+(0{\cdot}70)^2} = 0{\cdot}848 \text{ ft.};$$

from which we obtain

$$M = 0{\cdot}646 \text{ ft.}$$

And since the maximum value of the solar tide is 0·250 feet, we have, by equation (5),

$$\text{max. value of } 2S \sin \overline{2\sigma} = 0{\cdot}500 \text{ ft.},$$

and therefore

$$S = 0{\cdot}342 \text{ ft.}$$

Combining these results, we have as tide constants at Bunown,

1. Lunitidal interval $= 0^h\ 31^m$.
2. Solitidal interval $= 2^h\ 52^m$.
3. Age of lunar tide
   at high water $= 4^d\ 9^h$.
   at low water $= 4^d\ 9^h$.
4. Lunar coefficient $= 0{\cdot}646$ ft.
5. Solar coefficient $= 0{\cdot}342$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M} = 0{\cdot}529$.

The solar and lunar tides at Bunown were constructed from the foregoing constants, and compared with the observed tides. The results of the comparison are given in the following Tables:—

Bunown Tide, Table A.

Positive heights at high water for thirteen lunations, from 1851, January $10^d\ 16^h\ 42^m$, to 1851, December $31^d\ 11^h\ 36^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·41	0·39	+0·02	8	0·36	0·36	0·00
2	0·38	0·36	+0·02	9	0·25	0·33	−0·08
3	0·29	0·25	+0·04	10	0·21	0·21	0·00
4	0·12	0·13	−0·01	11	0·16	0·13	+0·03
5	0·20	0·20	0·00	12	0·23	0·20	+0·03
6	0·23	0·24	−0·01	13	0·31	0·30	+0·01
7	0·35	0·32	+0·03				

Mean difference = +0·006 ft.

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Bunown Tide, Table B.

Negative heights at high water for thirteen lunations, from 1851, January $10^d\ 16^h\ 42^m$, to 1851, December $31^h\ 11^h\ 36^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·45	0·48	−0·03	8	0·47	0·47	0·00
2	0·37	0·39	−0·02	9	0·40	0·39	+0·01
3	0·24	0·31	−0·07	10	0·26	0·26	0·00
4	0·30	0·24	+0·06	11	0·14	0·24	−0·10
5	0·37	0·28	+0·09	12	0·35	0·33	+0·02
6	0·43	0·40	+0·03	13	0·40	0·44	−0·04
7	0·49	0·44	+0·05				
Mean difference = 0·000 ft.							

Bunown Tide, Table C.

Positive heights at low water for thirteen and a half lunations, from 1851, January $2^d\ 10^h\ 54^m$, to 1852, January $2^d\ 17^h\ 39^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·50	0·52	−0·02	8	0·51	0·50	+0·01
2	0·38	0·45	−0·07	9	0·41	0·37	+0·04
3	0·23	0·35	−0·12	10	0·35	0·32	+0·03
4	0·40	0·30	+0·10	11	0·40	0·33	+0·07
5	0·40	0·36	+0·04	12	0·41	0·41	0·00
6	0·47	0·48	−0·01	13	0·44	0·50	−0·06
7	0·52	0·55	−0·03	14	0·50	0·52	−0·02
Mean difference = −0·003 ft.							

Bunown Tide, Table D.

Negative heights at low water for thirteen and a half lunations, from 1851, January $2^d\ 10^h\ 54^m$, to 1852, January $2^d\ 17^h\ 39^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·50	0·56	−0·06	8	0·40	0·50	−0·10
2	0·55	0·47	+0·08	9	0·47	0·46	+0·01
3	0·49	0·40	+0·09	10	0·40	0·43	−0·03
4	0·53	0·43	+0·10	11	0·48	0·46	+0·02
5	0·66	0·56	+0·10	12	0·60	0·57	+0·03
6	0·60	0·64	−0·04	13	0·50	0·60	−0·10
7	0·56	0·62	−0·06				
Mean difference = +0·003 ft.							

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Bunown Tide, Table E.

Difference of observed and calculated times of vanishing of diurnal tide at high water.

Age of lunar tide $=4^d\ 9^h$.

No.	Difference.	No.	Difference.
1	−0·25 lunar days.	14	−0·25 lunar days.
2	−0·20 ...	15	+0·60 ...
3	+1·60 ...	16	−0·20 ...
4	−0·20 ...	17	+0·55 ...
5	+2·75 ...	18	−0·45 ...
6	−0·20 ...	19	−0·25 ...
7	+0·75 ...	20	−0·20 ...
8	−0·20 ...	21	+0·80 ...
9	+0·50 ...	22	−0·20 ...
10	−3·00 ...	23	+0·65 ...
11	−1·20 ...	24	−1·20 ...
12	+1·15 ...	25	−0·25 ...
13	−0·20 ...	26	−0·75 ...
Mean = +0·001 lunar days.			

Bunown Tide, Table F.

Difference of observed and calculated times of vanishing of diurnal tide at low water.

Age of lunar tide $=4^d\ 9^h$.

No.	Difference.	No.	Difference.
1	−0·83 lunar days.	14	−0·08 lunar days.
2	−0·03 ...	15	−0·58 ...
3	+0·87 ...	16	+0·97 ...
4	−0·38 ...	17	−0·58 ...
5	+0·62 ...	18	+0·12 ...
6	+0·12 ...	19	+1·27 ...
7	+0·37 ...	20	−0·30 ...
8	−0·53 ...	21	+0·12 ...
9	−0·28 ...	22	+0·37 ...
10	+0·37 ...	23	−0·08 ...
11	−0·43 ...	24	−0·48 ...
12	−0·28 ...	25	−0·53 ...
13	−0·28 ...	26	+0·27 ...
Mean = +0·003 lunar days.			

From the foregoing Tables, it will be seen that there is an excellent general agreement between the observations and the tides calculated from the constants, and that these may therefore be relied on as very close approximations to the constants of the diurnal tide at Bunown.

[To be continued.]

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VI. *On the Effect of Chlorine in Colouring the Flame of Burning Bodies.* By D. Forbes, F.G.S., F.C.S., A.I.C.E.*

A CONSIDERABLE time back, whilst examining some saline minerals for boracic acid, and employing the usual test as to the power of colouring flame green, when treated with sulphuric acid and alcohol, it was found that a green flame presented itself, very similar to that which would be expected in case boracic acid were present in the minerals. On the most careful examination, however, no traces of boracic acid could be detected, and it was evident that the coloration of the flame must have proceeded from some other source.

As chlorine was present in considerable amount in the minerals in question, it became interesting to see whether its presence might have produced the green colour; and the experiments made on the subject fully confirmed this view. A number of further experiments on the power possessed by chlorine to colour flame, led to the following conclusions, which are stated briefly, as the results themselves sufficiently explain the *modus operandi*.

Chlorides treated with concentrated sulphuric acid and a very small amount of alcohol, produced green flames similar to those eliminated from borates under like treatment. Quantitatively, however, the flames were of less intensity; that is, the same weight of a borate would produce considerably darker green flames than when a chloride was used.

When chlorides were moistened with sulphuric acid and heated in the blowpipe flame, a faint green coloration was observed, which generally confined itself to the inner flame.

When hydrochloric acid is dropped cautiously on the flame of burning alcohol, a greenish tinge is observable.

A jet of chlorine or of hydrochloric acid gas directed upon the flame of a spirit-lamp or of coal-gas, produces a jet of green flame; this was also found to be the case when (by means of a convenient burner) chlorine gas was passed into the centre of a flame of burning coal-gas, or of vapour of alcohol.

When burning alcohol was injected into a globe filled with chlorine gas, the alcohol vapour continued burning at the mouth of the globe with a very flickering but often brilliant green flame.

From the above experiments, it will be seen that chlorine has in itself a decided colouring action on the flame of burning bodies, which may consequently in some cases lead to its being confounded with boron, as the green colour imparted to flame has hitherto been regarded as a most characteristic test of the latter element. When, as often happens, chlorine and boron occur together, this test consequently becomes nearly valueless.

* Communicated by the Author.

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VII. *On the Reciprocal Action of Diamagnetic Particles. Letter from* Prof. THOMSON *to* Prof. TYNDALL.

MY DEAR SIR, Glasgow College, Dec. 24, 1855.

I HAVE been prevented until to-day, by a pressure of business, from replying to the letter you addressed to me in the Number of the Philosophical Magazine published at the beginning of this month.

You ask me the question, "Supposing a cylinder of bismuth to be placed within a helix, and surrounded by an electric current of sufficient intensity; can you say, *with certainty*, what the action of either end of that cylinder would be on an external fragment of bismuth presented to it?"

In answer, I say that the fragment of bismuth will be *repelled* from either end of the bar provided the helix be infinitely long, or long enough to exercise no sensible direct magnetic action in the locality of the bismuth fragment. I can only say this with the same kind of confidence that I can say the different parts of the earth's atmosphere attract one another. The confidence amounts in my own mind to a feeling of *certainty*. In every case in which the forces experienced by a little magnetized steel needle held with its axis reverse along the lines of force, and a fragment of bismuth substituted for it in the same locality of a magnetic field, have been compared, they have been found to agree. In a vast variety of cases, a fragment of bismuth has been found to experience the opposite force to that experienced by a little ball of iron, that is, the same force as a little steel magnet held with its axis reverse to the lines of force; and in no case has a discrepance, or have any indications of a discrepance, from this law been observed. I feel therefore in my own mind a certain conviction, that even when the action is so feeble that no force can be discovered at all on the bismuth by experimental tests, such, in regard to sensibility, as have been hitherto applied, the bismuth is really acted on by the same force as that which a little reverse magnet, if only feeble enough, would experience when substituted in its place. Now there is no doubt of the nature of the force experienced by the steel magnet, or by a little ball of soft iron, in the locality in which you put the fragment of bismuth. One end of a magnetized needle will be attracted, and the other end repelled by the neighbouring end of the bismuth bar; and the attraction or the repulsion will preponderate according as the attracted or the repelled part is nearer. There is then certainly repulsion when the steel magnet is held in the reverse direction to that in which it would settle if balanced on its centre of gravity. In every case in which any magnetic force at all can be observed on a fragment of bismuth, it is such as the steel

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magnet thus held experiences. Therefore I say it is in this case repulsion. But it will be as much smaller in proportion to the force experienced by the steel magnet, as it would be if an iron wire were substituted for the bismuth core. Yet in this case the repulsion on the bismuth is very slight, barely sensible, or perhaps not at all sensible when the needle exhibits most energetic signs of the forces it experiences. You know yourself, by your own experiments, how very small is even the *directive* agency experienced by a steel magnet placed across the lines of force due to the bismuth core. You may judge how much less sensible would be the attraction or repulsion it would experience as a whole, if held along the lines of force; and then think if the corresponding force experienced by a fragment of bismuth substituted for it is likely to be verified by direct experiment or observation. I think you will admit that it is "incapable of verification," as well as "incontrovertible" by any collation of the results of experiments hitherto made on diamagnetics. As to the concluding paragraph of my letter which you quote, you do me justice when you say you accept it as an expression of my "personal conviction that the action referred to is too feeble to be rendered sensible by experiment." I will not maintain its unqualified application to all that can possibly be done in future in the way of experimental research to test the mutual action of diamagnetics under magnetic influence. On the contrary, I admit that no real physical agency can be rightly said to be "incapable of verification by experiment or observation;" and I will ask you to limit that expression to experiments and observations hitherto made, and to substitute for the concluding paragraph of my letter the following statement, written for publication three days later, and published in the same Number of the Magazine as that to which you communicated my letter (Phil. Mag. April 1855, p. 247). "The mutual influence" between rows of balls or cubes of bismuth in a magnetic field, "and its effects" in giving a tendency to a bar of the substance to assume a position along the lines of force, "are so excessively minute, that they cannot possibly have been sensibly concerned in any phænomena that have yet been observed; and it is probable that they may always remain insensible, even to experiments especially directed to test them."

I remain, my dear Sir,

Yours very truly,

Dr. Tyndall. WILLIAM THOMSON.

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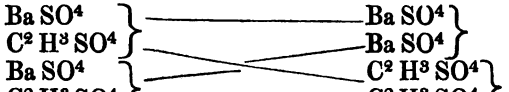
VIII. *On the Action of Water upon certain Sulphomethylates.* By Arthur H. Church, *Esq.**

In a former memoir† I have described the final products of the spontaneous decomposition of certain sulphomethylates: I there reserved for a future communication the full explanation of the reactions concerned. This explanation I believe I can now offer in the results of an experimental inquiry into the action of water upon the neutral sulphate of methyle.

Now as the sulphomethylate of methyle, obtained by the mutual action of iodide of methyle and sulphomethylate of silver, is not to be distinguished from the sulphate of methyle procured by the distillation of a mixture of methylic alcohol and sulphuric acid, we have a special reason, besides the general reasons universally known, for doubling the equivalent of sulphate of methyle, and representing it by the formula $C^4 H^6 S^2 O^8$. Then, too, it has been observed‡ that sulphate of æthyle, by boiling with water, yields together with alcohol an acid solution, in which, after neutralization with carbonate of baryta, sulphæthylate or isethionate (parathionate ?) and traces of methionate (?) of baryta are contained: also that sulphate of methyle undergoes analogous changes. Now we have seen that we may legitimately view the neutral sulphate of methyle as sulphomethylate of methyle. Hence the question arises, Is it not extremely probable, as the final organic products of the decomposition which sulphomethylate of baryta suffers are the same as those resulting from the similar decomposition of the sulphate of methyle, that this latter body is really produced by the action of boiling water upon the sulphomethylate of baryta? In this case of the sulphomethylate of baryta, it may be supposed that at first a double decomposition§ occurs between two equivalents, as represented in the following diagram:—



and, in the second place, that the sulphomethylate of methyle is transformed in the presence of water into methylic alcohol, and the stable sulphomethylic acid, according to the following equation:—

$$\left.\begin{matrix} C^2 H^3 SO^4 \\ C^2 H^3 SO^4 \end{matrix}\right\} + 2HO = C^2 H^4 O^2 + \left.\begin{matrix} C^2 H^3 SO^4 \\ H\ SO^4 \end{matrix}\right\}.$$

* Communicated by the Author.

† Phil. Mag. S. 4. vol. x. pp. 40-44.

‡ By Wetherill, *Ann. Pharm.* vol. lxvi. p. 117.

§ Or possibly the formula of ordinary sulphomethylic acid is $2(C^2H^42SO^4)$, while that of the β-acid is simply $C^2H^42SO^4$.

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Because of the identity in odour of sulphate of methyle and sulphomethylates undergoing decomposition, I am the more inclined to believe that sulphate of methyle is produced by the action of boiling water upon certain sulphomethylates, or in their spontaneous metamorphosis; although, indeed, in the first case it can exist at any one time in minute quantity only, on account of its almost immediate re-solution into methylic alcohol and sulphomethylic acid.

Of the truth of these ideas, which theory at first suggested, and experiment has since established, the present paper is intended to afford direct proofs: it also contains the results of an experimental inquiry as to the action of water upon sulphomethylate of æthyle. To this latter inquiry my attention was directed by certain theoretical considerations: the following questions suggested themselves for solution:—

Would

$$\left.\begin{matrix} C^2H^3SO^4 \\ C^4H^5SO^4 \end{matrix}\right\} + 2HO = C^2H^4O^2 + \left.\begin{matrix} C^4H^5SO^4 \\ H\ SO^4 \end{matrix}\right\}$$

$$\text{or} \quad = C^4H^6O^2 + \left.\begin{matrix} C^2H^3SO^4 \\ H\ SO^4 \end{matrix}\right\}$$

Or would the reaction be more complicated, two equivalents of sulphomethylate of æthyle being concerned in it, as in the reaction with sulphomethylate of baryta which I have already described; thus:—

$$\left.\begin{matrix} C^2H^3SO^4 \\ C^4H^5SO^4 \end{matrix}\right\} \qquad \left.\begin{matrix} C^2H^3SO^4 \\ C^4H^5SO^4 \end{matrix}\right\}$$

$$\left.\begin{matrix} C^2H^3SO^4 \\ C^4H^5SO^4 \end{matrix}\right\} \qquad \left.\begin{matrix} C^4H^5SO^4 \\ C^4H^5SO^4 \end{matrix}\right\}$$

And then in the second stage, the sulphomethylate of methyle and the sulphæthylate of æthyle decomposing in the way previously mentioned; thus:—

$$\left.\begin{matrix} C^2H^3SO^4 \\ C^2H^3SO^4 \end{matrix}\right\} + 2HO = C^2H^4O^2 + \left.\begin{matrix} C^2H^3SO^4 \\ H\ SO^4 \end{matrix}\right\}$$

and

$$\left.\begin{matrix} C^4H^5SO^4 \\ C^4H^5SO^4 \end{matrix}\right\} + 2HO = C^4H^6O^2 + \left.\begin{matrix} C^4H^5SO^4 \\ H\ SO^4 \end{matrix}\right\}$$

Although chloride of methyle is without action upon sulphomethylate of potash, yet if sulphomethylate of silver, iodide of methyle, and absolute alcohol*, be brought together and submitted in a sealed tube to a considerable temperature in the oil-bath, iodide of silver will form, and after opening the vessel some

* If water be present, methylic alcohol and sulphomethylic acid, together with a minute quantity of an indifferent oil, will occur instead of sulphate of methyle.

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quantity of crude sulphate of methyle may be distilled off. This liquid, as also the body obtained by the action of sulphuric acid upon methylic alcohol, when placed in water is slowly decomposed at ordinary temperatures, almost instantaneously at 100° C. The chief products of this reaction are methylic alcohol and β-sulphomethylic* acid. The acid produced differs from ordinary sulphomethylic acid. For if the acid solution† obtained by treating sulphate of methyle with water be saturated with carbonate of baryta, a beautiful baryta salt may be obtained from it by evaporation, or by precipitation with strong alcohol; the aqueous solution of this salt is not decomposable by ebullition, and the substance itself agrees in all its properties with the β-sulphomethylate of baryta which I have already described. A portion of this salt dried at 100° C., gave by ignition and subsequent treatment of the residue with nitric and sulphuric acids, the following numbers:—

·27 grm. salt furnished ·175 grm. $BaSO^4$ = 64·81 per cent.,

while theory, as $C^2H^3Ba\,2SO^4$, requires 64·9 per cent. $BaSO^4$. In whatever way the sulphate of methyle be prepared, and the transformation in contact with water effected, β-sulphomethylic acid is invariably produced. Only once, indeed, have I obtained in this reaction traces of a baryta compound of the same composition as the sulphomethylate, which was alterable at the boiling-point of water. The modified sulphomethylic acid is obtained from the methylic sulphate in a state of the greatest purity when water is allowed to act upon it at the ordinary summer temperature; when a greater degree of heat is employed, secondary decompositions occur. The methylic alcohol produced in the reactions described is usually contaminated at first with a trace of sulphate of methyle, imparting to it an alliaceous odour. As it is only by processes of purification, in which a considerable loss takes place, that dilute methylic alcohol can be rendered perfectly anhydrous, and so fit for analysis, I have contented myself with ascertaining that the distillate which came over between 66° and 68° possessed a most considerable inflammability; identifying it with methylic alcohol by a few qualitative experiments, and preparing a baryta salt of the acid produced by treatment of this dilute methyle spirit with sulphuric acid.

The salt employed in the analysis last given was prepared from the product of the reaction between sulphomethylate of silver and iodide of methyle. An analysis of the salt made from sul-

* For an account of this acid, which corresponds in the methyle series with the parathionic acid of the æthyle series, see Phil. Mag. S. 4. vol. x. pp. 40–44.

† Occasionally a trace of free sulphuric acid is present in this liquid.

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phate of methyle obtained in the usual way gave this result:—

·332 grm. salt, dried at 100° C., furnished by ignition ·2155 grm. $BaSO^4$ = 64·9 per cent. $BaSO^4$.

A brief account may now be given of the metamorphoses of the sulphomethylate of æthyle. Just as the product obtained by acting upon methylate of sodium with iodide of æthyle is not to be distinguished from the æthylate of methyle procured by the decomposition of æthylate of sodium by means of iodide of methyle, so sulphomethylate of æthyle is apparently identical with sulphæthylate of methyle, and yields the same products of decomposition when submitted to the action of water. To determine the nature of this reaction, I kept sulphomethylate of æthyle together with water in a closed vessel in a warm place. When the oil had entirely disappeared, and the liquid had lost its original odour, the more volatile portion was distilled off from a water-bath: a highly acid liquid remained in the retort, while the distillate was inflammable.

And now of this spirituous distillate. In the first experiment with sulphomethylate of æthyle, I attempted to concentrate the alcoholic product by means of recently burnt lime and anhydrous sulphate of copper. I obtained in this way a liquid almost absolutely anhydrous, and nine-tenths of which could be distilled over between 78° and 80° C. I was thus led to believe that sulphomethylate of æthyle is decomposed in presence of water, chiefly, if not entirely, into æthylic alcohol and sulphomethylic acid; and consequently expected to find that the analysis of a baryta compound prepared by neutralizing the acid residue remaining in the retort would give the numbers required by theory for the sulphæthylate of baryta. I accordingly made a determination of the sulphate of baryta in the salt obtained from the residue; it should be stated that the saturated aqueous solution of this salt was not alterable by continued ebullition:—

·411 grm. of salt gave ·2566 grm. of $BaSO^4$ = 62·43 per cent.

This analysis indicates a mixture of the sulphomethylate with the sulphæthylate of baryta nearly in the ratio 5 : 6. Other subsequent experiments seemed to show that the two acids are produced in equal equivalents. The apparent absence of methylic alcohol was soon accounted for. In fact it had entered into combination with the lime employed to deprive the alcohols of water; for by distillation, after the addition of water to this compound with lime, the methylic spirit was recovered, and after repeated treatments with anhydrous sulphate of copper a small quantity of the alcohol was obtained nearly pure, and boiling between 65° and 67° C.

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The more important conclusions to which my experiments have led me are these:—

I. That by the action of water upon sulphate of methyle, β-sulphomethylic acid is produced.

II. That in the action of water upon sulphomethylate of baryta, sulphate of methyle is formed, and that then the decomposition proceeds according to (I.).

III. That sulphomethylate of æthyle yields, by the action of water, methylic and æthylic alcohols, as well as β-sulphomethylic and parathionic acids.

December 1855.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. x. p. 456.]

June 21.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Enumeration of x-edra having an $(x-1)$-gonal Face, and all their Summits Triedral." By the Rev. Thomas P. Kirkman, A.M.

The object of the paper is to enumerate the x-edra which have an $(x-1)$-gonal face, and all their summits triedral; or, what is the same thing, to find the number of the x-acra which have an $(x-1)$-edral summit, and all their faces triangular.

Every x-edron having an $(x-1)$-gonal face has at least two triangular faces. Let A be an x-edron having all its summits triedral, and having about its $(x-1)$-gonal face k triangular faces. Suppose all these triangles to become infinitely small; there arises an $(x-k)$-edron B, having an $(x-k-1)$-gonal face, and all its summits triedral. B will have k' triangular faces, k' being not less than two, nor greater than k. And there is no other $(x-k)$-edron but B, which can arise from the vanishing of all the k triangles of A; *i. e.* there is no $(x-k)$-edron but B, from which A can be cut by removing k of the summits of B in such a way as to leave none of its k' triangles untouched.

If we next suppose the k' triangles of B to vanish, there will arise an $(x-k-k')$-edron C, having an $(x-k-k'-1)$-gonal face, all its summits triedral, and k'' triangular faces, k'' not <2, nor $>k'$. And thus we shall at last reduce our x-edron, either to a tetraedron, or to a pentaedron having triedral summits.

All x-edra here considered fall into six varieties, differing in the sequence of the $x-1$ faces that are collateral with the $(x-1)$-gonal base. They are either *irreversible*, as the octaedron 6435443, the seven faces about the base reading differently both backwards and forwards from every face; or *doubly irreversible*, as the heptaedron

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543543, whose six faces about the base are a repetition of an irreversible period of three; or *triply irreversible*, as the decaedron 643643643, whose faces exhibit a thrice-repeated irreversible period; or they are *reversible*, *doubly reversible*, or *triply reversible*, as the hexaedron 53443, the enneaedron 63536353, or the heptaedron 535353, exhibiting a single, double, or triple period, all reading backwards and forwards the same. If P_x be the number of x-edra having an $(x-1)$-gonal base, and all their summits triedral,

$$P_x = I_x + I_x^2 + I_x^3 + R_x + R_x^2 + R_x^3,$$

the symbols on the right denoting the numbers of x-edra of the six varieties that make up P_x.

Each variety is again subdivided according to the number of triangular faces. Thus, if $P(x, k)$ denote the number of x-edra on an $(x-1)$-gonal base, having k triangular faces, and all their summits triedral,

$$P(x,k) = I(x,k) + I^2(x,k) + I^3(x,k) + R(x,k) + R^2(x,k) + R^3(x,k).$$

The number k is not <2, nor $>\frac{x-1}{2}$, and $P_x = \Sigma P(x, k)$, for all values of k.

It is necessary to solve the following

Problem.—To determine the number of $(x+k+l)$-edra, none of which shall be the reflected image of another, that can be made from any x-edron having k triangular faces, by removing $k+l$ of its base-summits, thus adding $k+l$ triangular faces, so that none of its k triangular faces shall remain uncut.

The x-edron is supposed to have an $(x-1)$-gonal face, and all its summits triedral; no edge is to be removed, and $k+l$ not $>x-1$.

When the x-edron, the subject of operation, is *irreversible*, all the resulting $(x+k+l)$-edra will be irreversible. If it is *reversible*, some of them will be reversible and others irreversible; if it is *multiple*, some of them will be, and others will not be, multiple.

If the subject of operation is irreversible, the number required by the problem is

$$ii(x,k,l) = 2^k \cdot \frac{x-1-k^{l|-1}}{\lfloor l+1} - \Sigma_a (2^a - 1) \cdot 2^{k-a} \cdot \frac{k^{a|-1}}{\lfloor a+1} \cdot \frac{x-1-2k^{l-a|-1}}{\lfloor l-a+1},$$

taken for all values of a not greater than the least of k and l; i. e. $k-a$ not <0, 0 not $>l-a$.

The complete answer to the problem is expressed by the following equations, in which, of the capitals on the left, the first expresses the result, and the second the subject of operation. That is, $IR^2(x, k, l)$ denotes the number of irreversible $(x+k+l)$-edra having $k+l$ triangular faces about the $(x+k+l-1)$-gonal base, that can be cut from any doubly reversible x-edron having k triangles about its $(x-1)$-gonal base.

Whenever k or l in the function $ii(x, k, l)$ is not integer, the function, by a geometrical necessity, is to be considered $=0$.

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$\mathrm{II}(x,k,l)=ii(x,k,l)$,
$\mathrm{II}^2(2x+1,2k,l)=\frac{1}{2}\{ii(2x+1,2k,l)-ii(x+1,k,\frac{1}{2}l)\}$,
$\mathrm{II}^3(3x+1,3k,l)=\frac{1}{3}\{ii(3x+1,3k,l)-ii(x+1,k,\frac{1}{3}l)\}$,
$\mathrm{I}^2\mathrm{I}^2(2x+1,2k,l)=ii(x+1,k,\frac{1}{2}l)$,
$\mathrm{I}^3\mathrm{I}^3(3x+1,3k,l)=ii(x+1,k,\frac{1}{3}l)$;

$\mathrm{RR}(2x+1,2k,l)=ii(x+1,k,\frac{1}{2}l)$,
$\mathrm{RR}(2x+1,2k+1,l)=ii(x,k,\frac{1}{2}(l-2))$,
$\mathrm{RR}(2x,2k,l)=ii(x,k,\frac{1}{2}l)+ii(x,k,\frac{1}{2}(l-1))$;

$\mathrm{IR}(2x+1,2k,l)=\frac{1}{2}\{ii(2x+1,2k,l)-ii(x+1,k,\frac{1}{2}l)\}$,
$\mathrm{IR}(2x+1,2k+1,l)=\frac{1}{2}\{ii(2x+1,2k+1,l)-ii(x,k,\frac{1}{2}(l-2))\}$,
$\mathrm{IR}(2x,2k,l)=\frac{1}{2}\{ii(2x,2k,l)-ii(x,k,\frac{1}{2}l)-ii(x,k,\frac{1}{2}(l-1))\}$;

$\mathrm{R}^2\mathrm{R}^2(4x+1,4k,l)=ii(x+1,k,\frac{1}{4}l)$,
$\mathrm{I}^2\mathrm{R}^2(4x+1,4k,l)=\frac{1}{2}\{ii(2x+1,2k,\frac{1}{2}l)-ii(x+1,k,\frac{1}{4}l)\}$,
$\mathrm{RR}^2(4x+1,4k,l)=ii(2x+1,2k,\frac{1}{2}l)-ii(x+1,k,\frac{1}{4}l)$,
$\mathrm{IR}^2(4x+1,4k,l)=\frac{1}{4}[ii(4x+1,4k,l)+2ii(x+1,k,\frac{1}{4}l)$
$-3ii(2x+1,2k,\frac{1}{2}l)]$;

$\mathrm{R}^3\mathrm{R}^3(6x+1,6k,l)=ii(x+1,k,\frac{1}{6}l)$, $\mathrm{R}^3\mathrm{R}^3(7,3,3)=1$,
$\mathrm{I}^3\mathrm{R}^3(6x+1,6k,l)=\frac{1}{3}\{ii(2x+1,2k,\frac{1}{3}l)-ii(x+1,k,\frac{1}{6}l)\}$,
$\mathrm{RR}^3(6x+1,6k,l)=ii(3x+1,3k,\frac{1}{2}l)-ii(x+1,k,\frac{1}{6}l)$, $\mathrm{RR}^3(7,3,1)=2$,
$\mathrm{IR}^3(6x+1,6k,l)=\frac{1}{6}\{ii(6x+1,6k,l)+3ii(x+1,k,\frac{1}{6}l$
$-ii(2x+1,2k,\frac{1}{3}l)-3ii(3x+1,3k,\frac{1}{2}l)\}$,
$\mathrm{IR}^3(7,3,2)=\mathrm{IR}^3(7,3,1)=\mathrm{IR}^3(7,3,0)=1$;

$\mathrm{I}^n\mathrm{R}^m(x+1,k,x-k)=0$.

By the aid of the above, together with the following, equations, the $(x+k+l)$-edra having $k+l$ triangular faces, an $(x+k+l-1)$-gonal base and triedral summits, are successively found.

$$\mathrm{I}(x+k+l,k+l)=\Sigma\{\mathrm{I}(x+k')\,.\,\mathrm{II}(x,k',l')+\mathrm{I}^2(x,k')\,.\,\mathrm{II}^2(x,k',l')$$
$$+\mathrm{I}^3(x,k')\,.\,\mathrm{II}^3(x,k',l')+\mathrm{R}(x,k')\,.\,\mathrm{IR}(x,k',l')$$
$$+\mathrm{R}^2(x,k')\,.\,\mathrm{IR}^2(x,k',l')+\mathrm{R}^3(x,k')\,.\,\mathrm{IR}^3(x,k',l')\};\ \&c.\&c.$$

taken for all values of $k'+l'=k+l$.

Similar equations are to be formed for the remaining five subdivisions of $\mathrm{P}(x+k+l,k+l)$.

Of the products under Σ, the first factors are found by the preceding part of the process, and the second are given by the equations above written as solutions of the problem. The factors will of course frequently be zeros. Finally, if $x'=x+k+l$,

$$\mathrm{P}_{x+k+l}=\mathrm{P}_{x'}=\mathrm{P}(x',2)+\mathrm{P}(x',3)+\ldots+\mathrm{P}(x',\tfrac{1}{2}(x'-1)).$$

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Thus, to give an example,

$$P_{11}=P(11,2)+P(11,3)+P(11,4)+P(11,5)$$
$$=I(11,2)+I(11,3)+I(11,4)+(I(11,5)=0)$$
$$+I^2(11,2)+I^2(11,4)$$
$$+R(11,2)+R(11,3)+R(11,4)+R(11,5).$$
$$I(11,2)=I^2(9,2).II^2(9,2,0)+I(9,2).II(9,2,0);$$
$$I(11,3)=I(8,2).II(8,2,1)+R(8,2).IR(8,2,1)+I(8,3).I(8,3,0);$$
$$I(11,4)=I^2(7,2).II^2(7,2,2)+R(7,2).IR(7,2,2)$$
$$+R^2(7,3).IR^2(7,3,1);$$
$$I^2(11,2)=I^2(9,2).I^2I^2(9,2,0);$$
$$I^2(11,4)=I^2(7,2).I^2I^2(7,2,2);$$
$$R(11,2)=R(9,2).RR(9,2,0);$$
$$R(11,3)=R(8,2).RR(8,2,1);$$
$$R(11,4)=R(7,2).R(7,2,2)+R^2(7,3).RR^2(7,3,1);$$
$$R(11,5)=R(6,2).RR(6,2,3).$$

The result is

$$P_{11}=I_{11}+I^2_{11}+R_{11}=61+7+12=80.$$

"Notes on British Foraminifera." By J. Gwyn Jeffreys, Esq., F.R.S.

Having, during a great many years, directed my attention to the recent Foraminifera which inhabit our own shores, I venture to offer a few observations on this curious group, as Dr. Carpenter, who has favoured the Society with an interesting and valuable memoir on the subject, seems not to have had many opportunities of studying the animals in the recent state.

Rather more than twenty years ago I communicated to the Linnean Society a paper on the subject, containing a diagnosis and figures of all the species. This paper was read and ordered to be printed in the Transactions of that Society; but it was withdrawn by me before publication, in consequence of my being dissatisfied with D'Orbigny's theory (which I had erroneously adopted), that the animals belonged to the Cephalopoda; and my subsequent observations were confirmed by the theory of Dujardin. I have since placed all my drawings and specimens at the disposal of Mr. Williamson of Manchester, who has given such a good earnest of what he can do in elucidating the natural history of this group, by his papers on *Lagena* and the Foraminiferous mud of the Levant.

The observations which I have made on many hundred recent and living specimens of various species, fully confirm Dr. Carpenter's view as to the simple and homogeneous nature of the animal. His idea of their reproduction by gemmation is also probably correct; although I cannot agree with him in considering the granules which are occasionally found in the cells as ova. These bodies I have frequently noticed, and especially in the *Lagenæ*; but they appeared

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to constitute the entire mass, and not merely a part of the animal. I am inclined to think they are only desiccated portions of the animal, separated from each other in consequence of the absence of any muscular or nervous structure. It may also be questionable if the term "ova" is rightly applicable to an animal which has no distinct organs of any kind. Possibly the fry may pass through a metamorphosis, as in the case of the Medusa.

Most of the Foraminifera are free, or only adhere by their pseudopodia to foreign substances. Such are the *Lagena* of Walker, *Nodosaria*, *Vorticialis* and *Textularia*, and the *Miliola* of Lamarck. The latter has some, although a very limited, power of locomotion; which is effected by exserting its pseudopodia to their full length, attaching itself by them to a piece of seaweed, and then contracting them like india-rubber, so as to draw the shell along with them. Some of the acephalous mollusks do the same by means of their byssus. This mode of progression is, however, exceedingly slow; and I have never seen, in the course of twenty-four hours, a longer journey than a quarter of an inch accomplished by a *Miliola*, so that, in comparison with it, a snail travels at a railroad pace.

Some are fixed or sessile, but not cemented at their base like the testaceous annelids. The only mode of attachment appears to be a thin film of sarcose. The *Lobatula* of Fleming, and the *Rosalia* and *Planorbulina* of D'Orbigny belong to this division.

Dr. Carpenter considers the Foraminifera to be phytophagous, in consequence of his having detected in some specimens, by the aid of the microscope, fragments of *Diatomaceæ* and other simple forms of vegetable life. But as I have dredged them alive at a depth of 108 fathoms (which is far below the Laminarian zone), and they are extremely abundant at from 40 to 70 fathoms, ten miles from land and beyond the range of any seaweed, it may be assumed without much difficulty, that many, if not most of them, are zoophagous, and prey on microscopic animals, perhaps even of a simpler form and structure than themselves. They are in their turn the food of mollusca, and appear to be especially relished by *Dentalium Entale*.

With respect to Dr. Carpenter's idea that they are allied to sponges, I may remark that *Polystomella crispa* (an elegant and not uncommon species) has its periphery set round at each segment with siliceous spicula, like the rowels of a spur. But as there is only one terminal cell, which is connected with all the others in the interior by one or more openings for the pseudopodia, the analogy is not complete, this being a solitary, and the sponge a compound or aggregate animal.

I believe the geographical range or distribution of species in this group to be regulated by the same laws as in the Mollusks and other marine animals. In the gulf of Genoa I have found (as might have been expected) species identical with those of our Hebridean coast, and *vice versâ*.

In common with Dr. Carpenter, I cannot help deploring the excessive multiplication of species in the present day, and I would include in this regret the unnecessary formation of genera. Another

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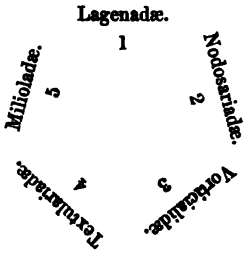
Linnæus is sadly wanted to correct this pernicious habit, both at home and abroad.

The group now under consideration exhibits a great tendency to variation of form, some of the combinations (especially in the case of *Marginulina*) being as complicated and various as a Chinese puzzle. It is, I believe, undeniable, that the variability of form is in an inverse ratio to the development of animals in the scale of Nature.

Having examined thousands (I may say myriads) of these elegant organisms, I am induced to suggest the following arrangement:—

1. *Lagena* (Walker) and *Entosolenia* (Williamson).
2. *Nodosaria* and *Marginulina* (D'Orb.), &c.
3. *Vorticialis* (D'Orb.), *Rotalia* (Lam.), *Lobatula* (Flem.), *Globigerina* (D'Orb.), &c.
4. *Textularia* (Defrance), *Uvigerina* (D'Orb.), &c.
5. *Miliola* (Lam.), *Biloculina* (D'Orb.), &c.

This division must, however, be modified by a more extended and cosmopolitan view of the subject, as I only profess to treat of the British species. To illustrate MacLeay's theory of a quinary and circular arrangement, the case may be put thus.



The first family is connected by the typical genus *Lagena* with the second, and by *Entosolina* with the fifth; the second is united with the third through *Marginulina*; the third with the fourth through *Globigerina*; and the fourth with the last through *Uvigerina*.

Whether these singular and little-known animals are Rhizopodes, or belong to the Amœba, remains yet to be satisfactorily made out.

London, June 18, 1855.

"Preliminary Research on the Magnetism developed in Iron Bars by Electrical Currents." By J. P. Joule, F.R.S.

The author had, many years ago, found that the magnetism developed by electro-magnetic coils in bars of upward $\frac{1}{3}$rd of an inch diameter, was nearly proportional to the strength of the current and the length of the wire, any alteration, within certain limits, of the diameter of a bar being attended with only trifling effects, so long as the point of saturation was not nearly approached. The Russian philosophers Lenz and Jacobi had, however, stated that the magnetism developed was, *cæteris paribus*, proportional to the diameter of the bar. The discrepancy between the above results is considered

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by the author to be owing rather to the different circumstances under which the experiments were tried than to any inaccuracies in the experiments themselves. Further, it appeared to him that in any case of induction by electric currents, careful distinction should be made between the several effects, which, compounded together, constitute the total magnetic action. Especially should a distinction be made between the magnetism existing under the inductive influence of the current and that permanently developed so as to remain after the electrical circuit is broken, and therefore the first efforts of the author were directed to ascertain the laws which regulate this permanent effect, or, as he thinks it may be conveniently termed, the *magnetic set*.

In his experiments the magnetism of any bar was ascertained, by placing it vertically with its lower end near a delicately suspended magnetic needle. This was a piece of sewing-needle $\frac{3}{16}$ths of an inch long, furnished with an index of fine drawn glass tube traversing over a graduated circle six inches in diameter. It was suspended by a filament of silk. The tangent of the deflection of the needle was found to be the exact measure of the attraction of a bar. In working with this instrument, it was found that the resistance of the air prevented the needle from swinging even once beyond the point of equilibrium to which it always arrived in less than ten seconds. This resistance of the air, so useful for bringing the needle rapidly to a state of rest, rendered it necessary to keep the entire instrument at a uniform temperature, for the slightest local application of heat produced currents of air within the glass case of sufficient strength to occasion considerable deflections. The circumstance points to the possibility of constructing a new and very sensitive thermometer which might be useful, particularly in experiments on the conduction of heat.

The method of experimenting consisted in observing,—1st. the magnetic attraction of any bar when a current circulated through its spiral; 2nd. the attraction still subsisting after the circuit was broken; 3rd. the attraction of the other pole of the needle on the reversal of the current; and 4th. the attraction remaining after this reverse current was cut off. The sum of the 1st and 3rd observations gives the total change in the magnetism of a bar by the reversal of the current. The sum of the 2nd and 4th gives the total permanent change of magnetism, or the *magnetic set*.

The experiments were made with iron bars of the several diameters, $\frac{1}{25\cdot6}$, $\frac{1}{17\cdot2}$, $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, and one inch, the length being in each case one yard; and also with iron bars $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$ and one inch diameter, of the length of two yards. In all the bars of small diameter up to $\frac{1}{4}$ of an inch, the magnetic set obtained by the use of feeble currents was found to be proportional to the square of the current employed in producing them. This law was found to subsist through a long series of electric intensities; but when the current was increased to a certain amount, the *set*, as observed in the bars of $\frac{1}{25\cdot6}$ and $\frac{1}{17\cdot2}$ of an inch diameter, increased in a much higher ratio, so as to vary, in some instances, with the 4th and 6th powers of

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the current. The point at which this phenomenon takes place is called the *magnetic breaking point*. A further increase of the current was attended with a rapid decrease of this ratio as the saturation of the bar was approached.

The total change of magnetic condition by reversal of the current, minus the magnetic set, is found to be nearly proportional to the intensity of the current.

Results of exactly similar character were obtained by the use of an electro-magnet, consisting of a bar of hard steel $\frac{1}{4}$ of an inch in diameter and $7\frac{3}{4}$ inches long.

In conclusion, the author points out the striking and instructive analogy which exists between the above phænomena and those of the set of materials as exhibited by Professor Hodgkinson, who, in his admirable researches, has proved that the *set*, or permanent change of figure, in any beam is proportional to the square of the pressure to which it has been exposed.

GEOLOGICAL SOCIETY.

November 5, 1855.—Mr. W. J. Hamilton, President, in the Chair.

The following communications were read:—

1. "On the Coal of the North-western Districts of Asia Minor." By Mr. H. Poole; communicated by the Foreign Office.

Mr. Poole, in his reports to the Government on the result of his journey to Asia Minor, to examine into the probability of workable coal being found in the country near Brussa and Ghio (Bithynia), in which coal had been reported to occur, states that he travelled from Ghio to the Lake Ascania, and around its shore, without finding any trace of coal; then from Yallova inland to Ortokoi, with like result. He next went from Yallova westwards along the coast as far as Kornikoi, where a bed of lignite, 9 inches thick, was worked to some extent by the Armenians four years since; thence he went inland to Sulmanli without seeing any indications of coal. In consequence of rumours of the existence of coal near the Lake of Apollonia, Mr. Poole travelled round that lake, but met with none. Mr. Poole next went from Yallova south-eastwardly to Tchougnoorkoi, where lignite, varying from 1 to 4 feet in thickness, and dipping at a high angle, has been also worked by the Armenians. This lignite is of no promise. Another excursion was to the Lake Sabandji, where a thin seam of lignite crossing the road on the south of the lake, and a lignite at Ag Sophé, to the east of the lake, were visited. Nowhere did Mr. Poole find proof of the existence of good workable coal in the districts visited.

2. "On the newer Tertiary Deposits of the Sussex Coast." By Mr. R. Godwin Austen, F.G.S.

From Brighton, westwards, between the chalk hills and the sea, the surface of the country is formed, first, by a raised terrace of "red gravels," lying on the sloping base of the chalk hills, and on the old tertiary deposits; secondly, the gravels of the Chichester levels, or

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the "white gravels." These latter are distinctly bedded and seamed with sand, and are more water-worn than the red gravels which pass under them; thirdly, the white gravels are overlaid by "brick-earth," which is somewhat variable in its characters. These, with their equivalents, are the glacial deposits of the district in question. The coast sections, though very limited in extent, exhibit several important phænomena illustrative of the history of these newer tertiary accumulations. At Selsea, where the glacial deposits are about 25 feet thick, the underlying eocene clay is seen, at extreme low water, to be perforated by a very large variety of *Pholas crispata*, and to be overlaid by a deposit containing *Lutraria rugosa*, *Bullastra aurea*, *Tapes decussata*, and *Pecten polymorphus*, contemporaneous with the Pholades. Elsewhere brown clays, or local ferruginous gravels, cover unconformably the eocene beds. The surface of the brown clay is deeply eroded, and bears a yellowish clay, which contains large chalk flints, and a great variety of pebbles and boulders of granitic, slaty, and old fossiliferous rocks, such as are now found in the Cotentin and the Channel Islands. One boulder of porphyritic granite measures 27 feet in circumference. A few sea shells (*Littorina*, &c.) occur in the yellow clay. This deposit the author regards as the equivalent of the "white gravel" in its extension southwards, the gravel having been littoral, and the clay with boulders a deposit formed in somewhat deeper water of this portion of the glacial sea. The coast-sections exhibit the surface of the yellow clay as having been eroded and covered by a variable deposit, sometimes gravelly and sometimes sandy, and containing marine shells (*Cardium edule*, *Ostræa edulis*, *Turritella terebra*, &c.). This band contains also fragments of the old crystalline rocks obtained from the destruction of the underlying yellow clay. On the shelly and pebbly band lies the brick-earth, an unstratified earthy clay deposit, with small fragments of flint and a few pebbles, and with occasional silt-like patches.

The particular subject of this paper was the occurrence of the granitic and slaty detritus in the yellow clay. These blocks are especially numerous near Bracklesham, Selsea, and Pagham. The author explained the difficulties that lie in the way of supposing that they were derived from the Cornwall coast, or direct from the shores of Brittany or the Channel Islands. His previous observations, however, on the bed of the English Channel had prepared the way for the explanation of the hypothesis he now advanced—of the former existence of a land-barrier, composed of crystalline and palæozoic rocks, crossing from Brittany to the south-east of England, and forming a gulf or bay open to the west. Into this bay the marine fauna represented by the *Pholas crispata* and its associates extended from the westward; and in the hollow of the bay, at a rather later period, coast ice brought the boulders from along the old shore line, which is now represented by a sunken peak in mid-channel and a shoal of granitic detritus. Alteration of level succeeded; and the partial destruction of the yellow clay deposit afforded the overlying pebble bed, and, in the author's opinion, the granitic blocks found in the old raised beach at Brighton. Mr. Godwin Austen

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thinks it probable that the superficial brick-earth of the district under notice was formed in a land-locked lagoon, subject to periodical freezing; and that the "elephant bed" at Brighton is one of its many and variable equivalents (in this case probably subaërial). The brick-earth area has been subsequently encroached upon by the estuaries of Pagham, Portsmouth, &c.; and the successive oscillations in the level of the land are evidenced in the estuarine deposits and submerged forests of Pagham, Bracklesham, Portsmouth, &c. With regard to the latest movements, the author's observations showed that from Lewes Levels to Chichester Harbour, and on to Hurst Castle, the coast exhibits signs of undergoing elevation at the present day. The coast of the Isle of Wight opposite seems on the contrary to be suffering depression, whilst the back of the island exhibits some curious signs of local oscillation.

November 21, 1855.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Notice of the Artesian Well through the Chalk at Kentish Town." By Joseph Prestwich, Jun., Esq., Sec. G.S.

The boring of this well has pierced the following succession of beds:—London Clay, 236 feet,—Woolwich and Reading series, 61½ feet,—Thanet Sands, 27 feet,—Middle Chalk (usually termed "Upper Chalk" in England), 244½ feet,—Lower Chalk, 227½ feet,—Chalk marl, 172 feet,—Upper Greensand, 59 feet,—Gault, 85 feet,—and then 176½ feet of a series of red clays with intercalated sandstones and grits. Altogether amounting to 1290 feet. It was expected that, in accordance with the general relations of the lower members of the Cretaceous series as they come to the surface in the districts North and South of London, that the sands of the Lower Greensand formation would be found immediately to succeed the Gault in the boring. Instead of the sands in question the red sandy clays have presented themselves, and the question of the probability of obtaining a supply of water by deeper boring depends upon the fact whether these red clays are a local variation of the Gault, and overlie the usual Lower Greensand, or whether the lower Cretaceous deposits have here put on a new character altogether. The very few fossils met with in the Clays speak strongly in favour of their being true middle Cretaceous lying above the horizon of the Lower Greensand; but the occasional occurrence in the clay of large rolled fragments of syenite, porphyry, basalt, hornstone, and old sandstone, and its general mineral features, seem to indicate a littoral character for these deposits, and to point to the possible neighbourhood of a ridge of older rocks, which have modified the conditions under which the lower cretaceous beds were formed in this area. The consideration of this important subject was referred to a Committee, who will report upon it at a future meeting of the Society.

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2. "On the discovery, by Mr. Robert Slimon, of Uppermost Silurian rocks and fossils near Lesmahago in the South of Scotland, with observations on the relations of those strata to the overlying Palæozoic rocks of that part of Lanarkshire." By Sir Roderick I. Murchison, V.P.G.S. &c.

The principal object of the author is to direct the attention of geologists to the recent discovery of the uppermost Silurian rocks of Scotland, in which country their presence was unknown. This important discovery was made by Mr. Robert Slimon of Lesmahago, who in the western part of that extensive parish of Lanarkshire detected very remarkable and large fossil crustaceans, the exhibition of which at the Glasgow meeting of the British Association induced Sir R. Murchison to visit the tract in question, accompanied by Professor Ramsay.

The descending order of the strata is well seen on the banks of the Nethaw river, Logan water, and other small streams; all tributaries of the Clyde. There the lower carboniferous rocks, composed of several bands of Productus and Encrinite limestone, frequent seams of coal and layers of ironstone, including the celebrated "black band," are underlaid by the Old Red Sandstone, as largely exposed between Lanark and Lesmahago. Towards its lower part the Old Red is marked by a powerful band of pebbly conglomerate; whilst its base is made up of alternating red and light greenish-gray flagstones and schists.

The latter are succeeded by dark gray, slightly micaceous, flag-like schists, charged with large crustaceans and other fossils, which organic remains, combined with the apparently conformable infraposition of the beds to the lowest Old Red, have led the author unhesitatingly to consider the Lanarkshire strata to be the equivalents of the uppermost Ludlow rock or the Tilestones of England.

These dark gray fossiliferous layers are underlaid by, and pass down into, a thick accumulation of similar mudstones, which becoming in some parts slightly calcareous, in others arenaceous, rise up into a district of round-backed moorland hills, ranging in height from 1600 to 2000 feet above the sea; the whole tract having been much penetrated by porphyries and other igneous rocks.

The uppermost Silurian rock of Lanarkshire contains a species of *Pterygotus* not to be distinguished from the species of that crustacean so abundantly found in the upper Ludlow rock of Shropshire and Herefordshire; like which the Scotch stratum holds the *Lingula cornea* and *Trochus helicites*? (Sil. Syst). The Lesmahago deposit is further characterized by the crustaceans of the group of Eurypteridæ (Burmeister), which are described by Mr. Salter under the name of *Himantopterus*. They are accompanied by another crustacean, the *Ceratiocaris*.

In conclusion, Sir Roderick pointed out the remarkable persistency of this zone of large crustaceans in various parts of the world; one of the Lanarkshire individuals has a length of *about* 3 *feet*! In Westmoreland (Kendal) the *Eurypterus* is found in the Tilestones, with many upper Ludlow fossils; in Podolia the stratum containing

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the *Eurypterus tetragonophthalmus* (Fischer) underlies Devonian rocks; and in the Russian Baltic island of Oesel, it has recently been detected by M. Eichwald in a limestone which had been referred by the author and his associates to the Ludlow rock. In North America the *Eurypterus* occupies the same geological horizon as in Russia and the British Isles; and it is to be remembered that large crustaceans of this group of Eurypteridæ have nowhere been found in rocks of older date than the Upper Silurian.

3. "Description of the Crustaceans from the Uppermost Silurian rocks near Lesmahago." By John W. Salter, Esq., F.G.S.

The large Crustacea referred to in the last paper were described by Mr. Salter. They belong to the family Eurypteridæ of Burmeister, and bear the closest relation to *Eurypterus*. They also present many analogies with the *Pterygotus*, particularly in the presence of a scale-like sculpturing on the body-rings, a character now known to be present in *Eurypterus*, and probably common to the whole family.

They are elongate crustaceans, with a comparatively short carapace, bearing the large sessile eyes on the margin (and not on the surface, as in *Eurypterus*), with ten or eleven body-rings unprovided with any appendages, and with a caudal joint either pointed or deeply bilobed. There are a pair of limbs adapted for swimming, a pair of maxillæ with serrated edges, and an anterior pair of long appendages with dilated bases, in all probability antennæ (*Eurypterus* has two pairs developed). From the strap-shaped or ligulate form of the swimming-feet, the name *Himantopterus* is proposed in contrast with *Eurypterus*, which has these organs dilated.

Five or six species were described; all new.

1. *H. acuminatus*; a foot long, with a mucronate caudal appendage.
2. *H. bilobus*; 7 inches long, the tail bifid.
3. *H. lanceolatus*, a smaller species, with a simply pointed apex.
4. *H. maximus*. The head only known; it must have been 3 feet long when perfect, and is the largest known. (*Pterygotus* may have been about the same size.)
5. *H.? simulans*. A large species, with very distinct sculpture.
6. *H. Banksii*. A small species from the Tilestones of Kington, Herefordshire; 3 or 4 inches long.

A note by Mr. Huxley, on the relations of these gigantic extinct Crustacea, showed that their zoological position was neither among the Phyllopods nor the Pœcilopods, nor intermediate between the Copepods and Isopods, as had been supposed, but that their structural peculiarities were to be paralleled only among the Cumoid Stomapods on the one hand, and the zoæform larvæ of the Macrura on the other. Drawings of a new genus of Cumoid crustacea, *Calyptoceros*, illustrated this position; and leaving out of consideration the Isopoda, Pœcilopoda, and Trilobita, it was shown that the Eurypteridæ exhibited the most rudimentary and larval forms of any known Crustacea.

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December 5, 1855.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Tilestones, or Downton Sandstones, in the neighbourhood of Kington, and their contents." By R. W. Banks, Esq. Communicated by Sir R. I. Murchison, V.P.G.S.

In the Bradnor Quarry near Kington, on the borders of Radnorshire and Herefordshire, the Tilestones and Downton Sandstone are seen to overlie the Ludlow rock in the following descending order:—1. thin tilestone; 2. wall-stone, 12 feet thick, unfossiliferous; 3. mudstone, 3 to 6 inches, coloured grey by the intermixture of vegetable matter, and containing fragments of *Pterygotus*, and other crustacean remains, together with fossils allied to *Cephalaspis Lyellii* and *C. Lewisii* (Agas.); 4. Downton sandstone, 3 to 4 feet, with *Lingula cornea*, *Trochus helicites*, *Pterygotus*, and Cephalaspis-like fossils as above; 5. another grey mudstone, similar in character and contents to No. 3; 6. yellow sandstone and flagstone, 4 feet, with the Cephalaspis-like fossils, *Pterygotus*, *Leptocheles*, and *Trochus helicites*; 7. Ludlow rock. Another section in the neighbourhood exhibits thin shaly beds of tilestone, with *Lingula cornea*, underlaid by layers of flattened *Orthonota amygdalina* and *Trochus helicites*, which rest on the equivalent of the Ludlow bone-bed, here about 2 or 3 inches thick, and containing *Orthoceras gregarium*, *O. politum*, *Goniophora cymbæformis*, *Orthonota amygdalina*, *Orbicula rugata*, *Holopella*, *Chonetes lata*, *Cornulites serpularius*, *Cucullella antiqua*, *Modiolopsis lævis*, *Rhynchonella*, *Bellerophon carinatus*, *Leptocheles*, *Onchus tenuistriatus*, *Sphagodus*, and *Serpulites*.

The organic remains of these tilestone, sandstone, and mudstone beds were illustrated by numerous highly finished drawings by the author; and these, together with his descriptive notes, indicated the existence of one or more hitherto unknown or little understood forms of Crustacean life, probably of the *Eurypteridæ* group, and elucidated several important characters in the carapace and appendages of the *Pterygotus*; with regard to which genus, Mr. Banks finds reason to differ from the generally received opinion that it was allied to *Limulus* and the Pœcilopods.

Mr. Banks's specimens of the fossils resembling *Cephalaspis Lyellii* and *C. Lewisii* offer considerable evidence towards invalidating the Ichthyic relationship of these fossils, and placing them amongst the *Crustacea*.

In conclusion—from the absence of the numerous *Mollusca* characteristic of the Ludlow rocks, and from the presence of *Crustacea* that have not been found in the Ludlow beds, and especially the abundance of the *Pterygotus*, so characteristic of the Middle Old Red of Scotland,—the author is inclined to separate these Downton or Tilestone beds from the Upper Ludlow Rocks, and class them (as Sir Roderick Murchison, previously to his later remarks on the subject, originally arranged them) as the bottom-beds of the Old Red Sandstone.

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2. "On the last Elevation of the Alps, with notices of the Heights at which the Sea has left traces of its action on their sides." By Daniel Sharpe, Esq., F.R.S. & F.G.S.

The object of this paper is to show that after the Alps had assumed their present form, the whole region was submerged below the sea, and stood 9000 feet lower than at present; and that it then rose out of the sea by a succession of unequal steps, separated by long intervals of time, during which the waves produced impressions on the sides of the Alps which are still visible. These effects are traced out under three heads: 1st. The erosion of the sides of the mountains, producing rounded forms which extend up to definite lines, above which the mountains rise into rugged peaks, in striking contrast with the smoother forms below. This change of form had been observed by Hugi, who referred it to different composition of the rocks; by Agassiz and Desor, who seeing that Hugi's view was incorrect, explained it by the action of moving ice, to which they arbitrarily assigned a definite upper limit; and lastly by Prof. J. Forbes, who has pointed out similar phænomena in Norway at 1500 or 2000 feet elevation. Mr. Sharpe shows that throughout Switzerland these lines of erosion occur at three definite levels of 4800, 7500, and 9000 English feet above the sea, and he argues that no action but that of water could have produced a uniformity of level over so large an area, and that it required a long period of time to have formed such deep indentations of the mountain sides.

2nd. The sudden change of steepness which occurs at the head of every Alpine valley is assumed to be due to the excavating action of water, standing for a long period at that height: and a table is given of the elevation above the sea of the heads of between forty and fifty valleys, at various altitudes, which shows a correspondence of level between valleys on the opposite sides of the Alps, and between the excavation of several valleys and the lines of erosion at 4800 and 7500 feet; while the ice and snow in the higher valleys prevent a comparison with the highest line at 9000 feet.

3rd. The terraces of alluvium in the valleys are considered, in accordance with the opinion of Mr. Darwin, Mr. Yates, and others, to have been formed by detritus carried down into water standing at the level of the head of the terrace. The elevation of many of these terraces is given, and a correspondence is shown of the height above the sea of terraces in valleys which have no connexion with one another, and of terraces in some valleys with the heads of other valleys.

All these effects might be produced by a sea surrounding the Alps, and cannot be otherwise explained; and the level of this sea being assumed to have been constant, the Alps must have been rising out of the waters while these operations were going on. The period of this, their last elevation, is stated to have been after the Tertiary epoch; and a great part of the vast accumulations of sand, gravel, and rounded blocks which are seen in the valleys of the Alps and covering the lowlands of Switzerland are considered to have been

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formed by the waves beating against the mountains during their elevation.

Lastly, referring to the angular erratic blocks on the sides of the Jura, &c., the author points out that he removes the only serious difficulty opposed to the views of those who have supposed them to have been transported by floating ice, by showing that the levels at which those blocks are found were below the sea for a long period at the epoch of their removal.

X. *Intelligence and Miscellaneous Articles.*

TWO PROCESSES BY WHICH THE PHÆNOMENON OF COLOURED RINGS MAY BE PRODUCED WITH GREAT INTENSITY. BY M. CARRÈRE.

WHEN a drop of a solution of bitumen of Judea in a mixture of benzine and oil of naphtha is let fall upon the surface of some water in a vessel, a very brilliant luminous phænomenon is seen to be immediately produced. The bituminous liquid extends regularly in a thin film on the surface of the water, and thus produces very bright colours. The colour furnished by the film changes every moment for a minute or two, because a portion of the benzine and oil of naphtha evaporates, and the thickness of the film diminishes. But in a little time the film itself is completely solidified, from the oxidizing action of the air.

This delicate solid film may be easily fixed upon paper. Thus, supposing that it has been produced in a tub at the bottom of which there is a socket with a stop-cock, and which also contains a stool, upon which, immersed in the water, rests the leaf of paper to be coloured; the film having been formed above the paper, all that is necessary to fix it on the paper is to open the stop-cock.

To obtain a regular coloration of the paper by means of the bituminous film, it is very important that the latter should be very coherent. I increased its cohesion by introducing a certain amount of caoutchouc into the solution of bitumen of Judea.

I also obtain the phænomenon of coloured rings with great brilliancy by exposing to the air hot and freshly-filtered common ink, in which sugar is the principal adhesive matter. This process may serve for the study of the phænomenon of coloured rings. In fact, as the thickness of the film which forms at the surface of the ink only increases very slowly, we may very easily and exactly determine the order in which the different tints produced by a homogeneous film succeed each other in proportion as its thickness augments. I have also succeeded in fixing on paper the film produced by ink; but as in this case the cohesion of the pellicle is very slight, I have only succeeded by taking the following precautions:—

1. I do not deposit the film upon the paper until it has acquired a great thickness.

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2. I choose bibulous paper to be coloured.

3. Before drying the paper, I impregnate it with a solution of gelatine.—*Comptes Rendus*, Dec. 10, 1855, p. 1046.

ON A NEW SEISMOMETER. BY M. KREIL, DIRECTOR OF THE IMPERIAL METEOROLOGICAL INSTITUTE, VIENNA.

The new seismometer (an instrument for determining the data concerning earthquakes), invented by M. Kreil, is a pendulum oscillating in every direction, but unable to turn round on its point of suspension, and bearing at its extremity a cylinder, which, by means of mechanism within it, turns on its vertical axis once in twenty-four hours. Next to the pendulum stands a rod bearing a narrow elastic arm, which slightly presses the extremity of a lead-pencil against the surface of the cylinder. As long as the pendulum is quiet, the pencil traces an uninterrupted line on the surface of the cylinder; but as soon as it oscillates, this line becomes interrupted and irregular, and these irregularities serve to indicate the time of the commencement of an earthquake, together with its direction and intensity.—*Proceed. Imp. Acad. Sciences, Vienna*, March 8, 1855.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1855.*

Chiswick.—November 1. Cloudy: frosty at night. 2. Overcast and cold: heavy rain. 3. Showery. 4. Fine. 5. Clear: dense fog: very fine: rain. 6. Fine. 7. Cloudy. 8. Constant heavy rain. 9. Slight fog: fine. 10. Foggy: very fine: foggy at night. 11. Very fine: cloudy. 12. Hazy. 13. Overcast. 14. Fine: frosty at night. 15. Frosty and foggy: very fine: dense fog at night. 16. Dense fog. 17. Fine: cloudy: rain. 18. Hazy: cloudy: rain. 19. Rain. 20. Drizzly: fine. 21. Overcast: rain. 22. Drizzly: overcast: fine. 23. Cloudy. 24. Cloudy and cold: showery. 25. Cloudy: clear: sharp frost at night. 26. Very fine. 27. Overcast: slight rain. 28. Overcast: cloudy: lunar rainbow at 10 P.M. 29. Overcast: cloudy. 30. Overcast: very fine.

Mean temperature of the month	40°·84
Mean temperature of Nov. 1854	39 ·35
Mean temperature of Nov. for the last twenty-nine years	42 ·95
Average amount of rain in Nov.	2·347 inches.

Boston.—Nov. 1. Fine. 2. Fine: rain P.M. 3. Cloudy: rain P.M. 4. Fine: rain A.M. 5. Cloudy: rain P.M. 6. Rain A.M. 7. Cloudy. 8. Cloudy: rain A.M. and P.M. 9. Fine. 10. Cloudy. 11. Fine. 12—15. Cloudy. 16, 17. Fine. 18, 19. Cloudy: rain P.M. 20. Cloudy. 21. Cloudy: rain P.M. 22, 23. Cloudy: rain A.M. and P.M. 24. Cloudy: rain P.M. 25. Cloudy. 26. Fine. 27. Cloudy: rain P.M. 28. Cloudy: rain A.M. and P.M. 29. Cloudy: rain P.M. 30. Cloudy.

* The observations by the Rev. C. Clouston of Sandwich Manse, Orkney, have not been received.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 9¼ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Nov.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
☾ 1.	29·704	29·667	29·36			44	27	34·5			n.	n.		.....	·40	
2.	29·727	29·579	29·38			47	34	36			nw.	n.		·49	.....	
3.	29·877	29·616	29·44			46	33	44			ne.	ne.		·03	·74	
4.	30·123	29·983	29·15			46	24	42			ne.	n.		·01	·14	
5.	30·180	30·161	29·77			50	32	41			w.	nw.		.....	.....	
6.	30·118	29·988	29·69			59	46	45·5			sw.	s.		·02	·10	
7.	29·857	29·662	29·40			55	43	49			sw.	s.		·02	·02	
8.	29·587	29·487	29·13			52	27	45·5			sw.	s.		·56	.....	
● 9.	29·801	29·751	29·40			59	40	36			s.	s.		·02	·06	
10.	30·045	29·807	29·44			58	36	47			s.	s.		.....	.....	
11.	30·226	30·164	29·75			58	44	39			sw.	s.		.....	.....	
12.	30·182	30·090	29·80			50	42	45			se.	s.		.....	.....	
13.	30·012	29·972	29·66			48	37	45			ne.	calm		.....	.....	
14.	30·008	29·949	29·55			46	24	41			ne.	nw.		.....	.....	
15.	30·168	30·084	29·69			47	22	41			w.	wnw.		.....	.....	
☽ 16.	30·234	30·187	29·87			42	27	27·5			sw.	w.		.....	.....	
17.	30·223	30·185	29·94			47	42	30·5			ne.	nw.		.....	.....	
18.	30·171	30·160	29·84			47	39	46·5			ne.	e.		·06	.....	
19.	30·069	30·010	29·78			47	38	42·5			e.	ene.		·05	·03	
20.	30·003	29·871	29·72			44	37	41			ne.	e.		·01	·01	
21.	29·889	29·835	29·58			43	36	40			ne.	e.		·02	.....	
22.	29·828	29·784	29·43			42	27	41·5			ne.	ese.		·01	·19	
○ 23.	29·764	29·750	29·43			43	34	37			sw.	calm		.....	·29	
24.	29·924	29·819	29·54			43	37	41			n.	nne.		·02	·20	
25.	30·277	30·043	29·82			43	18	39			ne.	ne.		.....	·14	
26.	30·289	30·241	30·00			42	36	33			ne.	n.		.....	.....	
27.	30·035	29·983	29·70			47	42	39			ne.	nnw.		·01	.....	
28.	30·112	30·019	29·70			47	36	43			ne.	nne.		·01	·07	
29.	30·111	30·045	29·75			45	33	41			n.	n.		.....	·01	
30.	30·078	29·980	29·63			45	26	36			w.	nw.		.....	·02	
Mean.	30·020	29·929	29·61			47·73	33·96	40·3						1·34	2·42	

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

FEBRUARY 1856.

XI. *Hydraulic Researches**. *By* G. MAGNUS†.

[With Two Plates.]

1. FEW subjects in physics have been so much experimented upon as the efflux of water from apertures made either in the bottom or in the sides of vessels. A glance at these numerous researches is sufficient to show that their object is for the most part a practical one,—the determination of the coefficients of efflux. Few have hitherto attempted to discover the physical conditions which give rise to the remarkable forms of water-jets issuing from differently formed apertures. This is the more remarkable, seeing that the phænomena in question are of daily occurrence; and we might have expected that the desire of understanding these phænomena would have, before this, suggested experiments calculated to explain them. Whilst the mysterious phænomena of electricity and the delicate phænomena of optics have been examined on every side and reduced to simple principles, the phænomena attendant upon the efflux of liquids not only lack explanation, but have not been even accurately observed.

2. The remarkable forms of liquid jets have often been described, particularly in the second half of the foregoing century by Michelotti, who, however, mentions them but briefly in his *Sperimenti idraulici*. Eitelwein has added to the German translation of this work, by Prof. Zimmermann (Berlin, 1808), several faithful drawings of these jets, without, however, entering upon their cause. George Bidone has published the most complete

* These investigations form a continuation of the Author's paper 'On the Motion of Fluids,' which appeared in the Phil. Mag. for January 1851.

† From Poggendorff's *Ann. der Ph. und Chem.* 1855, vol. xcv.

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investigation on this subject. In his memoir entitled "Experiments upon the forms and directions of Jets and Currents of Water*," the forms of fifty jets issuing from different openings are described. The same author gives an explanation of the production of their peculiar forms, which, however, has already been pronounced, and I think justly so, insufficient by Poncelet and Lesbros†. Bidone asserts that one of the most interesting of these phænomena depends upon an illusion. Some observers have asserted that peculiar spiral-shaped intersecting lines are visible on the surfaces of jets. With respect to these lines, Bidone says that the nodes and ventral segments which may be observed on the jets‡ continually approach one another when the pressure is diminished. Since the eye only momentarily perceives the continual changes thus produced in the positions of the glittering points, it was thought that the parts on the surface of the jet described spirals, and that the several threads of water crossed one another and passed from one side of the jet to the other. It is remarkable that Bidone has overlooked the conditions under which spiral-shaped lines are produced; nevertheless, without knowing these conditions, it is impossible to comprehend the complicated phænomena which jets of liquid present. Consequently, although Bidone's knowledge of the forms of jets is worthy of great praise, his explanations have not been accepted. My object is not to furnish a history of the published researches on the forms of jets. I will only further remark, that Poncelet and Lesbros have very accurately measured and described the form of a jet issuing from a vertical square orifice. As far as I know, this is the only exact measurement of a jet. But the authors of this celebrated research, after rejecting the explanation given by Bidone, did not furnish another calculated to explain the production of the remarkable forms which their measurements revealed. With respect to the beforementioned spiral-shaped lines they gave no explanation, but merely asserted that they cannot be illusory.

3. In order to study the form of a water-jet, it will, I believe, be found most convenient to examine first the action of two jets upon one another. It may appear strange to commence with the more complicated case; nevertheless, the nature of a water-jet, even when it issues from the simplest circular orifice, can easily become so complicated that the phænomena produced by the action of two jets upon one another are, if not simpler, at least more certain to observe.

* *Memorie dell'Academie di Torino*, vol. xxxiv. p. 229.

† *Expériences hydrauliques sur les lois de l'écoulement de l'eau*, par MM. Poncelet et Lesbros, p. 149.

‡ *Memorie dell'Academie di Torino*, vol. xxxiv. p. 346.

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Experiments on the mutual action of two Jets.

4. In these experiments I used an apparatus consisting of a zinc vessel containing about a cubic foot of water, in the bottom of which a glass tube, 15 millims. in interior diameter and $1{\cdot}^{m}3$ long, was fixed perpendicularly. At the lower extremity of this tube was a cock, and to this was affixed a brass tube with two branches inclined to each other at an angle of 30°, and provided with long tubes of vulcanized india-rubber. These tubes had also an interior diameter of 15 millims., and were provided with brass tubes of the same interior diameter and $0^{m}{\cdot}25$ long, to the anterior ends of which caps were attached whose thin walls were pierced to form the efflux orifices. These tubes were so disposed upon a fixed frame, that any required direction could be given to the water-jets issuing from them. Fig 1, Plate I., is a view of such an apparatus. In order to dispel the air lodged in the tubes of brass or india-rubber, and to prevent the admission of air through the orifice in the cap *i* during the preparation of an experiment, it was necessary to be able to close the tubes completely. For this purpose other caps, *qq'*, without orifices, were in readiness; these could be placed over those with orifices by sliding them over a thicker part of the tube at *pp'*.

As it was necessary during the experiments to be able to change slightly the direction of the jets, the tubes were fixed to a separate apparatus, by means of which this could be accomplished with perfect security. The brass tube connected at *w* with the caoutchouc tube was fixed, by means of the circular clasp *nn*, to the brass piece *lk*. The spiral screw *r* kept *k* from the piece *mm*, at a distance which could be regulated by the screw *s*. To the piece *mm* a cylindrical pin was fixed capable of turning in the socket *gg*. This socket was fixed by a clamp and screw, *zz*, to the frame. In this manner a horizontal or a vertical rotation could be given to the tube *wp*. In order to raise and sink it parallel to itself, a screw thread was cut on the socket *gg*, in which the ring *vv* could be screwed up and down. By this means it was possible slightly to raise or sink *mm*, together with its pin, in *gg*.

5. From the investigations of F. Savart*, it is known that when two jets, issuing with equal velocity from two equal circular orifices, whose axes fall in the same right line encounter one another at places where each is still unbroken, they form an almost circular surface perpendicular to the direction of the jets. Such a surface is shown in fig. 2.

6. When the axes of both jets, being still parallel, do not fall in the same right line, the water surface which is formed when they encounter one another is inclined to the direction of the

* *Annales de Chimie et de Physique*, 2nd series, vol. lv. p. 257.

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jets, and is no longer plane, but somewhat curved; neither is it circular, but extended for the most part in that direction in which it would be cut by a plane passing through the axes of both jets. Such a surface is shown in fig. 3, Plate I.

Savart does not appear to have observed this phænomenon, but only that which manifests itself when two jets of different diameters and parallel axes meet one another*. Neither has he occupied himself with the phænomena which are observable when the axes of the two jets are inclined to one another.

7. As it will often be necessary in what follows to distinguish the case when the axes of the jets are in the same plane from that when jets, whose axes do not intersect, encounter one another, I will, for the sake of brevity, call the encounter of two jets whose axes cut one another a central encounter, no matter whether they are parallel or inclined to one another.

Experiments with Jets of equal diameters.

8. In the following experiments, wherein the jets were always inclined towards each other, jets of different diameters were also employed; nevertheless, the phænomena attending the encounter of two jets issuing with equal velocities from two equal orifices shall be first described. When the axes of two such jets, *ab* and a_1b_1, fig. 4, lay in the same plane, that is to say, when a central encounter took place, a water surface was produced perpendicular to the plane passing through both axes; and if both jets had the same velocity at the place of meeting, the surface bisected the angle made by the jets.

This surface, however, was not equally distributed around the place of meeting, but was elongated principally in the direction of the motion of the jets. Fig. 4 shows the form of the surface when horizontal jets were employed.

9. The more acute the angle made by the jets, the smaller is the part *bq* of the surface within this angle. For instance, when the apertures from which the jets issued had a diameter of 3 millims., and the axes formed a right angle, the surface extended itself within the angle aba_1, so as to form the part *bq*, fig. 4. When the angle between the jets, however, amounted to 60°, the surface within the angle aba_1, as shown in fig. 5, no longer existed, neither was it observable when the angle was smaller.

The breadth *cd* of the surface *cdf*, figs. 4 and 5, also diminished with the magnitude of the angle made by the jets *ab* and a_1b_1. The angle being constant, the breadth of this surface diminished with the velocity of the water in the jets.

10. When the jets encountered each other at an angle, and

* *Annales de Chimie et de Physique*, 2nd series, vol. lv. p. 280.

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when their axes were not in the same plane, the phænomena were different according as the axes were more or less distant from each other. When they were so far apart that their edges only encountered each other, and when the angle which they formed was not greater than 60° or 70°, each jet, after contact, continued its path, although both deviated from their original directions and approached each other, so that at some distance from the place of their encounter they moved in almost parallel directions. At the same time a water surface was produced between them, *kb*, fig. 6, which ended in an irregular form at *k*.

11. When the angle made by the jets remained unaltered, but when by a parallel displacement of one jet their axes were brought nearer to one another, the phænomenon changed, so that the jets no longer moved parallel, but approached and again crossed each other on their paths; in such a manner, however, that the jet which at their first encounter was uppermost, was undermost when they encountered each other a second time. At the same time a surface of water formed itself between them, as shown in fig. 7, where *abcd* represents the one jet, and $a_1b_1c_1d_1$ the other; and where in the space $bcdc_1$, the water surface may be seen extended like a membrane.

When the angle between the jets was only about 30°, they crossed each other a second, and sometimes a third time, as in fig. 8; so that each jet formed a spiral-shaped line, and the spaces $bcdc_1$ and dd_1fe were filled with surfaces of water. In order to observe the phænomenon in this manner, the velocity of the water in the jets must not be too great, and they must have besides this a suitable diameter. In the phænomenon as shown in fig. 8, the orifices had a diameter of 3 millims., from which the jets issued so as to form an angle of 30°, and to touch each other only at their extreme edges. In order to obtain many such successive surfaces, the orifices from which the jets issue must be perfectly round, and at the same time the whole apparatus, and particularly the efflux tubes, must be immoveable. In this manner, however, about three surfaces can be obtained, as well when the jets *ab* and a_1b_1 before contact are vertical, as when they are horizontal.

12. Each of the water surfaces $bcdc_1$ and $defd_1$, fig. 8, appeared, as already mentioned, like a membrane stretched between the jets. They were not therefore plane, but of double curvature, and formed skew surfaces. Neglecting this curvature, however, as well as the parabolic curvature imparted by gravity to jets issuing from vertical orifices, the several surfaces were all nearly parallel to the axes of the jets *ab* and a_1b_1, when the latter encountered each other only at their extreme edges.

13. When the jets did not so encounter one another, but had,

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on the contrary, a larger cross section common to both, the inclination of the several surfaces towards each other changed, so that the first surface formed an angle with the one which was parallel to the jets *ab* and a_1b_1 before they encountered each other. This angle increased with the magnitude of the common section, and amounted to 90° when the latter was as great as possible, that is, when the encounter was a central one.

The second water surface ed_1f made with the first, and the third with the second, almost the same angle as the first surface $bcdc_1$ made with the plane parallel to the two jets. The second surface ed_1f, therefore, made with a plane parallel to the jets *ab* and a_1b_1, an angle equal to double that which bcc_1d made therewith; and if a third surface existed, it made an angle with a plane parallel to the jets of three times the above magnitude.

14. When, before contact, the axes of both jets were in one plane, in other words, when the jets made a central encounter, the first surface bcc_1d, as already mentioned, made an angle of 90° with the plane of the jets aba_1; the second water surface was parallel to the plane of the jets, and the third again at right angles thereto. These positions of the surfaces, however, were only visible when the velocity with which both jets met was rather small; in the latter case, however, the surfaces could be very distinctly seen turned at right angles to each other. Fig. 9 represents such a phænomenon as seen from one side and from above.

Explanations of the phænomena observable when Jets of equal diameters meet each other.

15. Before proceeding to describe other experiments, it will be best to give an explanation of those already described.

When two fixed bodies, for instance two perfectly equal spheres moving towards each other in the same right line with equal velocity, strike one another, they remain at rest provided they are inelastic; and when two more equal spheres follow the first two, they strike those already at rest with equal but opposite forces, and consequently are themselves also brought to rest. The deportment of liquid masses is different from this. When two perfectly equal water-jets, each of which may be considered as a series of particles of water following each other, move in opposite directions against each other so that their axes lie in the same right line, the first particles which strike one another are pressed against by those which follow, and in consequence of this pressure the water moves sidewards. As this action is continually renewed, a continuous lateral motion from the place of contact is sustained, and with equal velocity in all directions.

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In consequence of cohesion, the water moving sidewards forms an uninterrupted circular surface, fig. 2, which becomes thinner as the distance from the centre increases, until finally it is ruptured. The separated small masses of water gather into drops and fly off in the direction of the radii.

16. As long as the water surface is unbroken, two forces act on its parts. The one is the pressure of the approaching masses of water which propels them in the direction of the radii, the other is the cohesion which maintains it in connexion with the adjacent parts. In consequence of the latter, the velocity in the direction of the radius is diminished, for a part of the force which acts in this direction is expended in spreading the water out. When the continuity of the water surface therefore is ruptured by a solid body held at a small distance from its centre, the particles of water which pass this body are only drawn to one side by cohesion. The edges of the water surface, therefore, continually recede from this place of interruption. The curved edges which the water surface assumes in this case is shown in fig. 10, at $\alpha\beta$ and $\alpha\gamma$. As the particles of water moving along these edges are only connected with the water surface on one side, they are propelled in a tangential direction much further than are the particles of the water surface moving in a radial direction.

17. If the axes of both jets, which we still suppose to have equal transverse sections, lie in one plane, in other words, if a central encounter of two jets having equal velocities and forming an angle with each other takes place, it is evident that a water surface will likewise be produced, and that this will bisect the angle made by the jets, as shown in figs. 4 and 5. It is also evident that this surface cannot be equally extended in all directions; for if the water did not spread itself out, it would pursue its path so as to bisect the angle made by the jets, that is, in the direction of the resultant of both jets. When, besides this, each particle of water is propelled in a radial direction from the place where the jets meet, the several particles will move in curved lines approaching more and more to parallelism with that resultant. The surface, therefore, is extended principally in this direction. The small masses of water which separate themselves from the edges of the surface fly off in the direction of the tangents to their curvilinear paths. The smaller the angle aba_1 made by the jets, the greater the velocity of the water after contact in the direction of the resultant, and the smaller the pressure which causes the spreading out of the water. Consequently the breadth *cd* of the surface *cdf*, figs. 4 and 5, decreases with the angle aba_1, and the water also extends less in the direction *bq*, fig. 4.

18. The thickness of the surface also differs at different points. At the place where the jets meet, the lateral motion of the water

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has the greatest velocity on account of the pressure which the particles exert upon each other, consequently the surface is there thinnest. The velocity of the water in the direction *cd* decreases with the distance from the place of contact, and hence, owing to cohesion, the surface is thickest at such places. This effect is most apparent at the edges of the surface.

19. The cause of this contraction at the edges is easily explained. If we suppose that all the particles in the same surface move forward parallel to each other, and consider the effect of the mutual attraction of the particles which lie in a line perpendicular to the direction of motion; if we further suppose that the particles are separated from each other at equal distances, for example, if $abcd \ldots d_1 c_1 b_1 a_1$ represent these particles of which a and a_1 move at the extreme edges of the surface, then in consequence of the attraction between a and b they will approach each other, and for a similar reason b will approach c. But b cannot approach c without a simultaneously approaching c, for otherwise the distance between a and b would be greater than it originally was. The attraction between b and c necessitates the motion of the particles a and b, consequently b and c cannot approach one another as much as a and b can. In a similar manner, c and d cannot approach each other as much as b and c, or as a and b.

This consideration applies equally to all the particles of a section of the water surface perpendicular to the direction of translation, and it also holds good when the particles in such a section are in immediate contact with each other, only in this case they do not merely approach, but displace one another, so that the section in question is thicker at the edges than in the middle. This thickening of the surface often produces a sharply defined cylinder.

20. Not only the edges, but the whole water surface also is made thicker and thereby smaller; the edges approach one another, and thus the peculiar pointed form of the surface *dcf*, figs. 4 and 5, ensues.

As the edges, however, possess a certain quantity of motion, they either strike against one another with a corresponding force, or, if owing to any circumstance they have not both remained in the same plane, they pass one another and continue on their separate journeys. But if the edges encounter one another centrally, a new surface perpendicular to the last ensues. This process may be repeated several times, as shown in fig 9.

21. When the axes of both jets are not in the same plane, and when they encounter one another partially, the surface which is produced between them remains, in consequence of cohesion, connected with both jets. The latter would pursue their paths, and continue to recede further from each other if they were not

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prevented by their connexion with this surface. The continual contraction of the intermediate surface deflects the jets from their direction, but the former thereby becomes thinner and is ultimately ruptured. The jets then continue to move onwards in the same direction as the one they had when they separated from the surface.

22. If the force with which the jets move is not sufficient to rupture the surface, either because the angle between them or their velocity is too small, or because only a small portion of them pursues the same path, the greater having served to form the water surface, the jets are continually brought nearer to one another by the force of cohesion, and the water surface appears unbroken, as shown by $bcdc_1$, in fig. 7.

But inasmuch as the axes of the jets before contact were not in one plane, they do not meet one another now; that which was before on one side, *e. g.* the right or above, lies on the other side, the left or below, at the next contact. For if we imagine a plane parallel to the axes of both jets immediately before contact and equally distant from both, the force with which the homologous points of both jets seek to approach one another may be divided into two equal parts, of which the one acts in the above plane, the other perpendicular to it. The latter causes the two corresponding points of the jets in this plane to approach one another, and therefore moves both in opposite directions. Hence the jets arrive in the above plane, but in consequence of their inertia they pass beyond it; and thus the jet which formerly lay on one side of the plane now lies on the other, and *vice versâ*. This process is repeated, and thus the closed surfaces *bcd* and dc_1ef, fig. 8, are produced.

23. When the jets encounter merely at their edges, only a small part of them is employed to form the water surface. The greater part continues its path, and consequently remains almost entirely in the plane which is parallel to the axes of the jets before contact, fig. 7.

For the same reason, the second and third water surfaces, when they exist, also remain nearly parallel to this plane.

24. When, however, a more complete contact between the jets takes place, the position of the surface changes; for the impinging masses would form a surface perpendicular to the plane parallel to both jets before contact, were they not connected with the parts of the jets which continue their journey in this plane. It is evident, however, that if the masses of these parts of the jets are small in comparison to the mass of the water surface, the direction of the latter will determine that of the jets themselves. And as this surface, if unconnected with the jets, would be perpendicular to the plane passing through the axes, it is evident

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that the water surface will form a greater angle with this plane the smaller the masses of the jets in comparison to that of the water forming the surface, until the angle finally becomes a right angle. It is equally manifest that the water surface makes a small angle with that plane if the mass of the jets preponderate, and that if the jets only meet at their edges, the angle almost vanishes.

The same process which takes place at the first meeting of the jets repeats itself when they pass over each other for the second time, and there partially encounter. But because those parts of the jets which meet at this second place are in the same proportion to the entire diameter of the jet as at the first encounter, the second surface, ed_1f, fig. 8, makes the same angle with the first, bcc_1d, as this does with the surface parallel to the axes of the jets ab and a_1b_1, and finally the third makes again the same angle with the second.

25. If the axes of the jets are in the same plane, and if they encounter centrally, but at a small angle, and with moderate velocity, the extent of the surface, which is now at right angles to the original plane of the jets, is only small; for the particles of water in this surface are only impelled in the direction of the radius with a small force. In that case the thick edges principally are formed, as observed above, § 18. These act on each other like two jets, and produce by their encounter a new water surface, which is at right angles to the plane of the edges, that is, at right angles to the direction of the first water surface. This process is repeated until the force employed in spreading out the water so diminishes the force with which the edges act on each other in the third or fourth surface, that no further appreciable expansion takes place.

26. The production of the rounded edges of surfaces, their encounter, and the new surfaces produced thereby, together constitute the phænomena which are influential in the remarkable forms assumed by jets issuing from angular openings.

Experiments with Jets of unequal diameters.

27. The phænomena presented by the meeting of two jets issuing from circular openings of different diameters, and whose axes are in the same right line, have been described by Savart, not only when both jets issue at equal pressures, but also when the pressures are unequal. These experiments, described in Savart's interesting treatise *Sur le Choc de deux Veines Liquides**, are of minor importance for the present purpose. He has not described the phænomena presented by jets of different diameters meeting at an angle; indeed the subject of the encounter of jets

* *Annales de Chimie et de Physique*, vol. lxv, p. 257.

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which form an angle with each other has not at all engaged his attention.

28. When, in the apparatus described in § 4, the cap with an aperture of 3 millims. was changed for one with a diameter of 6 millims., the tubes being horizontal, and at an angle of 60°, the phænomenon represented in fig. 11 *a* and *b* (looked at from above and at the side), showed itself: The surface *cdf* appeared curved, and had its concavity on the side on which was the thinner jet. The surface no longer bisected the angle which the two jets formed, but approached the direction of the thicker jet, as is seen in fig. 11 *b*. If the axes of both jets were not exactly in one plane, if, for instance, that of the thinner jet were a little higher than that of the thicker, the convexity was turned somewhat downwards, and the concavity could be seen when looked at from above, as shown in fig. 11 *b*.

29. If the direction of the horizontal tubes remained the same, but if one of them, that for instance which furnished the thinner jet, were raised parallel to itself until the edges only encountered, then the phænomenon shown in fig. 12 was presented.

A water surface *cdf* was again formed, but it was almost plane and horizontal, having the form *cdf*. From the edge *df* small drops flew off in large quantities in a tangential direction, which was not the case at the edge *cf*.

Explanation of the phænomena presented by the encounter of two Jets of different diameters forming an angle with each other.

30. The form of the surfaces resulting from the encounter of jets of unequal diameter may be easily explained from a consideration of the process in the case of jets of the same diameter.

We have seen, § 28, that when two jets of equal diameters, whose axes are in the same plane, meet each other, they produce a water surface which is at right angles to the plane of the axes *ab* and a_1b_1, fig. 11, and whose direction bisects the angle which these two jets make with each other. But since the mass of the thicker jet is more considerable than that of the thinner, it results that the surface formed by the union of both jets follows more the direction of the thicker one. That part of the thicker jet which is met by the thinner experiences the strongest deviation from its direction. The edges, on the contrary, which do not directly encounter, suffer also a smaller deviation. Thus is produced the concave form of the surface, and its deviation from the line *pq*, which bisects the angle made by the two axes.

31. If the thinner jet met the thicker only at its edge, § 29, both, after their encounter, would retain their directions if the surface formed between them, and spread out like a membrane, *cdf*, fig. 12, did not hinder it. Since this surface, in consequence

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of cohesion, contracts, that is, becomes thicker and narrower, it brings the rays nearer together. It is, however, manifest that by this mutual approximation the thinner is far more deviated from its direction than the thicker, because a greater quantity of motion is present in the latter. Hence results the peculiar form of the edge *df*.

32. Between the form of the surface which fig. 11 represents and that represented by fig. 12, occur all possible variations, according as both jets intersect more or less centrally. Besides this, the forms of the surfaces are changed, as much by an alteration in the relative diameters of the jets as by the change in the angle at which they meet, or in the distance of the place of contact from the outlet. But all these forms may be explained in a similar manner to §§ 30 and 31.

Experiments with Jets which issue under different pressures.

33. It has been assumed in the preceding, that both jets issued at the same pressure. By narrowing one of the caoutchouc tubes in the apparatus described in § 4 the pressure could be regulated, as thereby the afflux was lessened.

Savart has described the action of jets which move towards each other in the same line under different pressures*, hence the present treatise will be confined to jets which meet at an angle. If both issue from equal circular orifices, a water surface is formed as in the case already described; this does not, however, bisect the angle formed by the two jets, but follows more the direction of that which moves with the greater velocity. The surface is also not plane, but concave on the side of the jet with the greater velocity.

34. Since the surface follows the direction of the resultant of both jets, § 17, it is manifest that it does not bisect the angle of the jets. The reason that it appears concave is as follows:—Let us suppose that both jets are divided into strata of equal thickness, which are parallel to the axes, and at right angles to the plane going through both axes. Then the corresponding strata of both jets, which lie on the interior side of the angle, meet sooner than the rest. The interior strata have therefore already formed a surface before the next strata encounter. But the place in which the succeeding strata encounter is evidently not in the surface resulting from the preceding strata, but more towards the side of the surface having the smaller velocity. Hence the middle of the surface already formed would be pressed towards this place. This process is repeated for all successive strata, and

* *Annales de Chimie et de Physique*, 2nd series, vol. lv. p. 257.

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thereby the surface produced from the whole strata becomes concave on the side of the jet with the greater velocity.

35. Since the phænomena which jets of different velocities present, when they proceed from apertures of different magnitudes, and meet at different angles, are of minor importance for the following considerations, and may be all explained in a similar manner, it would not be worth while to enter upon them further at present.

Experiments on the form of Jets which issue from apertures in thin plates.

36. I turn now to the description of experiments on the form of jets produced when water issues from apertures in thin plates.

In these experiments the apertures were made in the bottom of the vessel, because the form of jets issuing from such apertures are more regular than those issuing from the side. It is an indispensable condition for the regular form of a jet, that the water should flow towards the aperture from all sides with equal velocity. This is not to be attained with apertures which are in the side of a vessel. By using very high columns of water during the efflux, the influence of the unequal pressures which are exercised at the upper and under sides of a vertical aperture may be made as small as possible, so that those differences of pressure appear infinitely small; but the pressures cannot be made so equal as in the case of apertures in the bottom of a vessel. But in order to obtain a regular afflux from all sides, it is not sufficient to make the apertures in the bottom of the vessel. First of all, it is necessary that the bottom should be horizontal and quite plane; next, that the vessel should be so regular and of such dimensions that the side walls do not hinder the motion of the water; and finally, that no external influence whatever should produce any motion in the water.

37. And even if all these conditions are fulfilled, the afflux will always be irregular for a short time after the efflux has begun; for there results a rotatory motion in the liquid, which after some time not only communicates itself to the greater part of the lower strata, but also to the higher strata of the liquid. Such a motion results whenever all the particles of water in one plane, for instance those at the bottom of the liquid, have no common resultant. No rotation takes place if the directions of motion of all the particles at the bottom goes through *one* point, the centre of the aperture; but if by any disturbing influence, such as an obstruction at the bottom, or a motion communicated to the water by external agency, the directions of the individual particles are altered, then a rotatory motion of the liquid must ensue, for all the directions do not pass through the same point.

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Such disturbing influences are unavoidable, and hence the liquid after some time commences a rotation, which, beginning in the lowest strata, is soon communicated to the whole mass. In consequence of this rotation, a funnel-shaped cavity is formed, which at first is only seen on the surface, but soon draws itself downwards to the aperture.

38. In order to prevent this funnel-shaped cavity, and indeed all rotation, I used a contrivance consisting of four very thin tin plates, each $0^m{\cdot}25$ high, and $0^m{\cdot}12$ broad, which, as is seen in fig. 13, are joined at right angles to each other in the form of a windlass. It must be so placed on the bottom of the vessel, that the efflux orifice may be exactly in the middle of the space which remains free between the vertical plates. By this means, rotation, except in the immediate neighbourhood of the efflux orifice, is prevented without hindrance to the regular afflux of the water. In what follows, I shall call this contrivance the *tranquilizer*.

39. The water issued under changeable pressure. This kind of efflux was preferred to that under constant pressure, because it is not possible to keep the level of a liquid constant without disturbing the regularity of its motion. In order that the water might not issue too rapidly, and the changes of pressure take place too quickly, the vessel was made of such dimensions, that, when full, it took more than a quarter of an hour to empty it even when the largest efflux orifices were used, and more than half an hour when the smaller orifices were employed.

40. This vessel was made of strong zinc plate, $0^m{\cdot}4$ high and $0^m{\cdot}8$ in diameter. In order that the bottom might remain straight, it was strengthened underneath by a plane wooden board $0^m{\cdot}02$ thick.

In order to be able conveniently to attach the various efflux orifices, a brass plate $0^m{\cdot}21$ in diameter and 5 millims. thick, was so placed in the bottom of the vessel that its inner surface was a little lower than the rest of the bottom. This brass plate had in its middle an aperture $0^m{\cdot}05$ in diameter. Its inner surface was ground perfectly plane, and upon it was fastened, water-tight, a tin plate 1 millim. thick, also quite plane, in the middle of which was the efflux orifice. The tin plate had the same diameter as the brass plate, so that its orifice was right in the middle of that of the brass plate. When the two were fixed, the superior surface was level with the zinc forming the rest of the bottom. It was thereby possible to keep the bottom of the vessel quite plane, and to prevent any hindrance to the motion of water, especially in the neighbourhood of the efflux orifice. As a mixture of wax and oil was sufficient to fasten the tin plate on the polished brass one, it was easy to attach plates with orifices of various dimensions. At the under surface of the brass

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plate an arrangement was made by which the aperture could be closed.

Fig. 14, Plate II., represents a section of the middle part of the bottom of the vessel, a quarter of its natural size; vv_1 is the wooden board used to strengthen the bottom, xx_1 is the tin plate with its efflux orifice z. This is placed on the brass plate qpp_1q_1, which is soldered at q and q_1 to the zinc bottom rr_1. The aperture of this plate pp_1 was closed by means of a brass plate provided with a handle w, which, on its upper side, where it pressed against the plate qq_1, was covered with leather. In order to fasten it, two pegs, yy_1, were fixed in it horizontally at places opposite each other, which pegs fitted into two hooks oo_1. When the plate was turned, the pegs passed out of the hooks, the plate could be removed, and the water flowed out.

41. The vessel thus arranged stood upon a firm frame of strong wood, $1^m{\cdot}75$ high. The lower part was joined by strong cross bolts, while the junction at the upper part was effected by a few iron bands. It was thereby possible to observe the jet by full light at the efflux orifice, where its form was particularly interesting.

In the middle of the four legs of the frame the vessel for the reception of the water was placed. This was unconnected with the frame.

42. When the water fell in freely, a tolerably loud noise resulted; for the water already fallen in, and through it the vessel itself, were moved. In order to avoid this motion, a board was placed almost vertically in the vessel. As it was placed so that the water must fall on that part of it which was out of the vessel, it slid equably down the board, and neither noise nor any observable motion resulted.

43. In turning now to the description of jets which issue from differently formed apertures, it might be expected that I should commence with the circular aperture as the simplest. Since, however, the object of the present treatise is the explanation of the peculiar forms of jets which proceed from different apertures, I prefer to give the description of the few jets most useful for this explanation in the order which seems most fitted for the attainment of my object. I begin, therefore, with that from a long rectangular orifice.

Jets from an elongated rectangular orifice.

44. When an elongated rectangular orifice, 2·6 millims. long by 25 millims. broad, was used, the jet took the form which is represented in Plate II., when all disturbing influences were avoided, and the efflux took place regularly. Fig. 15 *a* is the view obtained on looking at the jet at right angles to the greater

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dimension AB of the orifice, and fig. 15 *b* that when looked at at right angles to the narrow side BC of the aperture*.

45. The water contracts very strongly after it has left the orifice. Hence result the roundish edges A*e* and B*f*, which are very sharply defined towards the rest of the surface. These produce, when they encounter centrally, the first surface *fg*, which is at right angles to AB*ef*. This surface *gf* assumes at its lower half strong edges again, and by the encounter of these the following surface *gh* is formed, which is again at right angles to the preceding *gf*. Thus the process is repeated, and many such similar surfaces are formed successively, just as in § 14, in the case of the central encounter of two separate jets, which formed an angle with each other.

46. The dimensions of these surfaces alter with the pressure under which the water issues. The greater the pressure, the longer the surfaces, *i. e.* the greater the distances *fg*, *gh*, &c. Their breadth *ik*, i_1k_1 is also changed, but not in the same proportion as the length. Hence it is, that, under greater pressure, the surfaces appear more extended, *i. e.* their breadth is less in proportion to their length. The distance from the efflux orifice to the first contraction of the jet at *ef* changes also with the pressure, and is greater when the pressure increases. Fig. 15 *a* and *b* represent the surfaces under a pressure of $0^{m}{\cdot}15$.

47. If, while the efflux orifice remains unchanged, the afflux of water from two opposite sides be made unequal, by putting, for instance, a piece of tin plate in the vessel near the long side CD of the aperture upon the plate in which this is made, the jet deviates from the vertical. It is bent then towards the side CD near which is the tin plate. This bending or deviation from the vertical is the stronger the thicker the plate is. With the exception of this bending, the form of the jet is just that described in § 45, provided that the plate hinders the afflux of water equably in the whole length of CD.

If instead of bringing the hindrance on a broader, it be brought on a narrower side of the aperture, for instance on the side AD, the jet will deviate towards this side. Altogether the deviation is towards that side from which the afflux takes place with less rapidity. If the hindrance does not extend all the length of the side in question, it is obvious that the deviation which the stream experiences when the afflux is hindered upon one of the narrower sides, is far less than when the hindrance is on one of the longer sides. This deviation not being considered, the form of the jet remains in both cases just as if no such hindrance to the afflux had been there.

48. If hindrances to the afflux be brought on both the narrow

* In order to make this more intelligible the cross sections α, β, &c. are added.

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sides of the aperture AD and BC, the shape of the jet remains quite similar to that which is formed without any hindrance; even if there be placed on each of the narrow sides, close to the edge, high vertical walls, which are many, perhaps a hundred times, broader than these sides of the aperture. In fig. 15 *c* the lines PP_1 and QQ_1 represent the position of such vertical walls or screens near the narrow sides of the aperture ABCD. Only that part of the jet from the orifice to the greatest contraction changes its form and assumes the figure represented in fig. 15 *d*. The place of the greatest contraction, *ef*, lies further from the efflux orifice, and the limits of the jet A*e* and Bf_1 appear up to this contraction less curved than in the case of the jet formed without these screens.

49. If screens be used in the manner represented in ground plan by RS and R_1S_1, fig. 15 *e*, the jet next to the afflux orifice assumes the form fig. 15 *f*. The boundary lines A*e* and B*f* near the efflux orifice are almost at right angles. They only begin to converge at a greater distance from the orifice, so that the part *ef* of the greatest contraction is still further from it than in the preceding case, fig. 15 *d*. Below this place of greatest contraction, the form of the jet is quite similar to that represented in fig. 15 *a* and *b*.

50. It follows hence, and the correctness of the conclusion has been proved by experiment, that even if an aperture be made in a thick plate, the form of the jet remains almost unchanged; for when the water issued through a tube which was 20 millims. long, and which had a cross section equal to the elongated aperture employed in the thin plate, the jet had just the same form as when the walls set up at the narrow sides of the aperture, § 48, were used. Care must be taken that the issuing water quite filled the tube. Then the distance from the aperture to the greatest contraction appeared, as before, longer than in the case of the orifice in the thin metal plate without any walls.

51. In using the aperture in the thin wall, if the afflux for the half of one of the long sides, as CD, be prevented, not by a wall, but by a piece of metal plate, E, fig. 16, half as long as the side CD, which is so placed near it, as is represented in fig. 16, a considerable change in the form of the jet takes place. It appears now spirally wound. It is at the same time deviated from the vertical, and towards that side on which is the piece of metal E. This is attempted to be shown in fig. 16 *a* and *b*. Fig. 16 *a* represents it as seen by one who stands before the long side of the aperture AB, and fig. 16 *b* as seen by one who stands before the narrow side BC.

52. For as soon as the liquid has left the orifice, two thick edges Af_1 and B*f* are formed, as in the case where no hindrance

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is present, §§ 44, 45; they do not, however, encounter centrally. Hence the jet assumes, as seen in the drawing, a form exactly similar to that described in § 11, and represented in fig. 8, Plate I., which results when two separated jets meet at an angle, but not centrally. Each of the edges moves in a spiral line about the other, and between them the water surface is extended as a membrane. The edges may move at a tolerable distance from each other, and need not meet, as was necessary in the case of two separated jets, § 11.

53. The more considerable the hindrance at E, the more spiral-shaped does the jet appear; for with increasing breadth of the hindrance, the edge $Af_1g_1h_1$ deviates more towards D, where the hindrance is placed. At the same time the two edges not only remain at a considerable distance from each other, but it occurs sometimes that the mass of water between them bursts, and that then each continues its own path, like two separated jets. The phænomenon is then just like that described in § 11, and depicted in fig. 6, Plate I., which is seen at the encounter of two separated jets.

54. Instead of employing a hindrance at E, a spiral-shaped jet may be obtained in several other ways. It is only necessary that both the thick edges which are formed as soon as the mass of water has left the orifice, should not encounter centrally. If, for example, the aperture be so altered that it is not everywhere of equal breadth, but something of the form of fig. 16 *c*, the jet will have the previously described, § 51, spiral form. It also does not fall vertically, but is deflected a little towards the side upon which the aperture is broader.

55. If the narrower part of the aperture is situated, as in fig. 16 *d*, just in the middle of the broader part, the jet is not spiral-shaped, but has a similar form to that described in §§ 44 and 45, and depicted in fig. 15 *a* and *b*, which was obtained from an aperture having everywhere the same breadth, and in which there was no hindrance to the afflux.

56. There are, doubtless, besides the hindrance produced by the metal plate E, § 51, and the changed form of efflux orifice, § 54, other kinds of hindrances and changes of form of the efflux orifice, by which the spiral shape of the jet can be produced. But even with an orifice of perfectly regular form, and in the absence of any hindrance, the jet assumes the spiral shape, especially when such a motion is by any cause produced in the water as to render the afflux to the aperture irregular.

57. It has been already remarked, § 37, that a rotatory motion occurs whenever a special arrangement, such as the tranquilizer described in § 38, is not employed. It was also explained there why the water assumes such a motion.

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If it be assumed that the axis about which the rotation takes place goes through the centre of the aperture, the water particles which are in the one half of this aperture will be moved by the rotation in an opposite direction to those in the other half. In consequence of this rotation, the particles, after they have left the aperture, do not fall vertically, but deviate from the vertical; those in the one half deviate towards the one side, and those in the other towards the opposite side. Hence the edges of the jet are not in a plane, and encounter either not at all, or at any rate not centrally. Hence the jet assumes, as remarked above, the spiral shape, fig. 16 *a* and *b*.

58. If the rotation in the vessel be very violent, the two edges deviate, soon after the liquid has left the vessel, so strongly in opposite directions, that the water mass between them separates, and both edges continue their way as two separate jets, moving often at a considerable angle to each other.

59. The ease with which the rotation of the liquid in the vessel occurs, or rather the difficulty of avoiding it without using the tranquilizer, is the reason why the jet in its regular form, fig. 15 *a* and *b*, has hitherto been seldom observed.

60. Phænomena quite analogous to those which the jet from an elongated rectangular orifice presents, are seen if the water be allowed to flow in a similar thin layer over the edge of a vessel; but the forms which are then presented are seldom regular.

The different forms of a jet issuing from an elongated quadrilateral aperture, which have been described in the preceding sections, afford a *point d'appui* for the peculiar forms presented by other apertures.

[To be continued.]

XII. *Analysis of a Babylonian Cylinder and Amulet.* By John Spiller, *Metallurgical Laboratory, Museum of Practical Geology**.

In the British Museum there is a large collection of engraved cylinders, seals, &c. of Assyrian workmanship, the results of the researches of Layard, Rawlinson, and Loftus. They are chiefly composed of natural mineral substances, such as rock-crystal, agate, carnelian, serpentine, lapis-lazuli, &c.; there is also one of shell, and another of glass. There are two varieties, however, of which the composition was doubtful, and the following analyses were made for the purpose of determining their chemical constitution. The specimens operated upon were received directly from Mr. W. Kennett Loftus.

* Communicated by the Author.

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The Babylonian "Cylinder."

The general form of this is cylindrical, slightly diminishing in diameter towards the middle, with a hole drilled through the centre lengthwise, for the purpose, no doubt, either of stringing them, or supporting them on a central rod, after the manner of the cotton-reels in a spinning-machine. Their external surfaces are beautifully polished, and around the cylinders are very finely-executed intaglio engravings of human figures or deities, together with the arrow-headed characters, which, in the case of the cylinder analysed, had been almost entirely erased. The great hardness of the materials of the cylinders must have necessitated the possession by the Assyrians of well-tempered engraving instruments. Such tools have been found, and were, in fact, in the course of transmission to this country, together with numerous other antiquities intended for the French Government, but were unfortunately lost by the overturning of a barge in the rapids of the river Euphrates. These gravers were of hardened steel, and described as well fitted for their purpose.

The "cylinder" from which a portion was broken off for analysis weighed 338·91 grains, measured in length 1·19 inch; the diameter at either extremity was ·6 inch, but lessening towards the middle of the cylinder, as before stated; the bore of the longitudinal drill hole was ·125 inch. This, as well as another cylinder of very similar appearance and specific gravity, was found to attract strongly the magnetic needle; in this respect, and in the character of the fracture, it bore a close resemblance to the native varieties of magnetic oxide of iron, but the chemical analysis was required to confirm this supposition.

The Amulet.

The cylinders were accompanied by an amulet or charm, the weight of which was 127·92 grains; it had the form of an elongated, tolerably regular ellipsoid, measuring 1·5 inch in length by ·34 inch greatest width; near one extremity was a transverse perforation very sharply drilled. This specimen had the colour and general appearance of hæmatite, and did not exert any visible action on a delicately suspended magnetic needle. Its fracture was minutely crystalline, and very compact.

In elucidation of the subjoined tabulated results, it is only necessary to state that the iron was estimated by a standard solution of bichromate of potash, a process which a considerable experience in the analysis of iron ores has shown to give very accurate results; the iron and manganese were separated by boiling with an alkaline acetate; the phosphoric acid by a modification of the method proposed by Fresenius. Besides the con-

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stituents enumerated, they were both tested for chromium and zinc; the oxide of the former metal, being isomorphous with sesquioxide of iron, might possibly have been present as in the mineral chrome iron. Zinc also is known to occur in Franklinite, a mineral of the magnetic oxide type, in which part or the whole of the protoxide of iron is replaced by the corresponding oxide of zinc. Neither of these metals, however, were found to be present.

Babylonian.

	Cylinder.	Amulet.
Peroxide of iron . . .	94·57	97·14
Protoxide of iron . .	3·91	trace
Oxide of manganese . .	trace	·12
Lime and magnesia . .	...	traces
Phosphoric acid . . .	·08	·24
Water	·56	·08
Insoluble residue . . .	·72	2·62
	99·84	100·20

Insoluble residue.

Silica	·53	2·55
Alumina	·19	·07
Peroxide of iron . . .	trace	
	·72	2·62
Specific gravity . . .	4·94	5·02

Formula . $(10Fe^2O^3 + Fe^2O^3, FeO)$. Fe^2O^3.

From this statement, it will be seen that the materials of the cylinders and amulet agree in chemical composition, as in hardness and specific gravity, with known varieties of native magnetic oxide of iron and hæmatite; their manufacture from these minerals cannot therefore be any longer a matter of doubt.

London, January 9, 1856.

XIII. *On a Method of exhibiting fine Galvanometric Experiments to a large audience; extracted from a Letter from* M. du Bois Reymond *to* M. Magnus*.

WE have so often spoken together of the difficulty of showing fine galvanometric experiments to a large audience, that I believe it will interest you to learn that I have at length succeeded in completely surmounting this difficulty. I have been able to show my finest experiments on animal electricity,

* Translated from Poggendorff's *Annalen*, vol. xcv. p. 607.

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such as the experiments with the nerve current, and the negative variation of the muscular current in the living human body, to a numerous audience in the theatre of the Royal Institution.

The means made use of readily suggest themselves. They consist in furnishing the magnetic system with a reflecting surface, by which a bundle of parallel rays are reflected and thrown upon a screen, the motions of the luminous image being observed instead of those of the needle itself. When the light is sufficiently intense, the moving image can of course be shown to any number of hearers, and it is also manifest that an almost unlimited sensibility is attainable by this method. Helmholtz and I had already conversed on this subject; indeed he informed me by letter, that he had successfully applied the method several years ago in repeating my experiments. He made use of a galvanometer of tangents devised by himself, which is essentially the same as that since empirically found by Gaugain, and the principle of which has been developed by Bravais. A reflecting steel disc supplies, according to Weber's suggestion, the place of the bar-magnet and mirror originally suggested by Poggendorff. Making use of sunlight, Helmholtz thus succeeded in showing the principal phænomena of the muscular current.

I had placed at my disposal a multiplying galvanometer of 28,780 coils, constructed by Sauerwald of Berlin, according to my directions, for my friend Dr. Bence Jones. For this instrument I obtained from the same artist an astatic system of thicker needles than ordinary, united together by a piece of brass, instead of the tortoise-shell which I generally use. A prolongation of the uniting piece above the upper needle carries an extremely light ring of brass, at the upper point of which is the eye made use of to suspend the system. Within the ring moves upon a horizontal axis a mirror, fixed in a brass setting of extreme lightness. The mirror, which I owe to the kindness of M. Schieck, consists of a small round glass cover, made use of for microscopic purposes, silvered at one side, and possessing a diameter of 19·5 millims. Two simple cocoon fibres are sufficient to sustain the whole securely. The ring, in which the mirror may be inclined towards the horizon, can itself be turned round the prolongation of the brass joining piece; so that it is thus in our power, by properly deflecting the system, to reflect the incident beam of light in any required direction. It is, however, to be remarked, that when the inclination of the mirror towards the horizon augments, the sensibility of the arrangement diminishes.

As light-source, an electric lamp from Duboscq in Paris, also belonging to Dr. Bence Jones, was made use of. It was fed by a battery of forty cells of Grove, and our friend Prof. Tyndall had the kindness to undertake the regulation of the light, and

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the direction of the ray upon the mirror. At first a diaphragm was placed before the converging lens of the lamp, so that the light struck the mirror alone, for I feared that the warming of the air about the galvanometer might produce currents, and thus disturb the needle. It was found, however, that this precaution was unnecessary, and that the full luminous sheaf might be permitted, without prejudice, to fall upon the multiplier. In this way it was much easier to cast a sufficient quantity of light always upon the mirror, which, when the diaphragm was applied, was a work of some difficulty; for although the lamp was placed nearly in the plane of the system, it was necessary to keep it at a certain distance from the multiplier, so that its own magnetic power exercised no influence on the needle. With the distance, however, it is plain that the difficulty of keeping the ray exactly on the mirror increased in like proportion.

On a screen about 2·5 metres in length, a vertical black line marked the position of the luminous image when the needle was undeflected. The distance of the mirror from the screen was about 3 metres. The sensitiveness of the arrangement was rather too great than too small; as the muscular current drove the needle and mirror against the stops, while a deflection of even 11° was sufficient to cause the image to leave the screen. It was beautiful to observe, under the influence of the electromotive force of a minute nerve, or a few muscular fibres of a frog, the luminous image flying through the lecture room. . . . .

XIV. *On the Solar and Lunar Diurnal Tides of the Coasts of Ireland. By the* Rev. Samuel Haughton, *Fellow of Trinity College, Dublin.*

[Continued from p. 64.]

Section VI. *Diurnal Tide at Rathmullan.*

HAVING separated the solar and lunar tides in the diurnal tide at Rathmullan, I obtained the following results:—

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights = 0·29 ft.
2. Maximum value of lunar tide for negative heights = 0·47 ft.
3. Maximum value of solar tide = 0·23 ft.
4. Diurnal solitidal interval = $9^h\ 40^m$.
5. Age of lunar tide = $5^d\ 10^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights = 0·13 ft.
2. Maximum value of lunar tide for negative heights = 0·20 ft.
3. Maximum value of solar tide = 0·23 ft.
4. Diurnal solitidal interval = $9^h\ 40^m$.
5. Age of lunar tide = $4^d\ 20^h$.

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Adding together the first two of each of the preceding series of values, we find—

Range of lunar tide at high water $=0{\cdot}76$ ft.
Range of lunar tide at low water $=0{\cdot}33$ ft.

Hence by equation (3),

$$\cot(m-i_m)=\frac{0{\cdot}76}{0{\cdot}33}=\cot(23^\circ\ 28'),$$

which, converted into time, gives

$$m-i_m=1^h\ 37^m;$$

but m, the moon's hour-angle at high water, is, in Rathmullan time, $5^h\ 43^m$, and therefore

$$i_m=4^h\ 6^m.$$

By equation (4), we have

$$\text{max. value of } 2M\sin 2\mu=\sqrt{(0{\cdot}76)^2+(0{\cdot}33)^2}=0{\cdot}829 \text{ ft.};$$

from which we obtain

$$M=0{\cdot}632 \text{ ft.}$$

And since the maximum value of the solar tide is 0·23 feet, we have, by equation (5),

$$\text{max. value of } 2S\sin 2\sigma=0{\cdot}46 \text{ ft.},$$

and therefore

$$S=0{\cdot}315 \text{ ft.}$$

Combining together the preceding results, we obtain the following diurnal tidal constants for Rathmullan:—

1. Lunitidal interval $=4^h\ 6^m$.
2. Solitidal interval $=9^h\ 40^m$.
3. Age of lunar tide
   at high water $=5^d\ 10^h$.
   at low water $=4^d\ 20^h$.
4. Lunar coefficient $=0{\cdot}632$ ft.
5. Solar coefficient $=0{\cdot}315$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M}=0{\cdot}498$.

The solar and lunar tides were carefully constructed from the preceding constants, and compared with the observed tides. The results of this comparison are given in the following six Tables.

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Rathmullan Tide, Table A.

Positive heights at high water for fifteen lunations, commencing 1850, November $5^d\ 12^h\ 21^m$, and ending 1851, December $22^d\ 10^h\ 45^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·34	0·42	−0·08	9	0·45	0·49	−0·04
2	0·50	0·53	−0·03	10	0·45	0·46	−0·01
3	0·57	0·49	+0·08	11	0·44	0·36	+0·08
4	0·55	0·43	+0·12	12	0·40	0·31	+0·09
5	0·32	0·31	+0·01	13	0·40	0·32	+0·08
6	0·20	0·31	−0·11	14	0·30	0·39	−0·09
7	0·40	0·39	+0·01	15	0·44	0·48	−0·04
8	0·40	0·50	−0·10				

Mean difference = −0·002 ft.

Rathmullan Tide, Table B.

Negative heights at high water for fifteen and a half lunations, commencing 1850, November $5^d\ 12^h\ 21^m$, and ending 1852, January $5^d\ 9^h\ 45^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·58	0·61	−0·03	9	0·60	0·67	−0·07
2	0·70	0·67	+0·03	10	0·60	0·67	−0·07
3	0·62	0·69	−0·07	11	0·83	0·59	+0·24
4	0·60	0·67	−0·07	12	0·74	0·52	+0·22
5	0·72	0·51	+0·21	13	0·44	0·51	−0·07
6	0·36	0·49	−0·13	14	0·57	0·60	−0·03
7	0·50	0·57	−0·07	15	0·62	0·67	−0·05
8	0·55	0·64	−0·09	16	0·68	0·67	+0·01

Mean difference = −0·003 ft.

Rathmullan Tide, Table C.

Positive heights at low water for fifteen lunations, commencing 1850, November $6^d\ 16^h\ 15^m$, and ending 1851, December $21^d\ 4^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·14	0·09	+0·05	9	0·14	0·17	−0·03
2	0·10	0·15	−0·05	10	0·11	0·20	−0·09
3	0·13	0·19	−0·06	11	0·21	0·24	−0·03
4	0·26	0·23	+0·03	12	0·13	0·16	−0·03
5	0·06	0·18	−0·12	13	0·15	0·16	−0·01
6	0·20	0·06	+0·14	14	0·18	0·10	+0·08
7	0·21	0·10	+0·11	15	0·19	0·22	−0·03
8	0·10	0·10	0·00				

Mean difference = −0·003 ft.

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Rathmullan Tide, Table D.

Negative heights at low water for fifteen and a half lunations, commencing 1850, November $6^d\ 16^h\ 15^m$, and ending 1852, January $4^d\ 8^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·13	0·07	+0·06	9	0·16	0·16	0·00
2	0·40	0·23	+0·17	10	0·14	0·24	−0·10
3	0·11	0·20	−0·09	11	0·26	0·28	−0·02
4	0·10	0·23	−0·13	12	0·20	0·24	−0·04
5	0·20	0·23	−0·03	13	0·03	0·14	−0·11
6	0·13	0·20	−0·07	14	0·18	0·09	+0·09
7	0·25	0·08	+0·17	15	0·12	0·12	0·00
8	0·30	0·10	+0·20	16	0·16	0·23	−0·07

Mean difference = +0·002 ft.

The preceding Tables, A, B, C, D, show the agreement in height between the observed and calculated tides at Rathmullan. The following Tables, E, F, show the differences between the observed and calculated times of vanishing during the fifteen and a half lunations.

Rathmullan Tide, Table E.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide = $5^d\ 10^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·43	12	+0·43	22	−0·07
2	+2·93	13	−1·27	23	−0·97
3	−1·27	14	+0·43	24	−0·57
4	−0·07	15	−1·57	25	+2·03
5	−1·37	16	+0·43	26	+1·63
6	+0·93	17	−2·07	27	+0·93
7	−2·57	18	+0·43	28	+2·43
8	+1·63	19	+2·13	29	−0·67
9	−1·17	20	+0·43	30	−0·67
10	+1·93	21	−1·77	31	+1·27
11	−1·77				

Mean difference = +0·001 days.

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Rathmullan Tide, Table F.

Difference of observed and calculated times of vanishing at low water at Rathmullan, expressed in lunar days.

Age of lunar tide = $4^d\ 20^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	−1·0	12	+0·3	22	−1·7
2	+2·4	13	+0·3	23	−1·3
3	−1·4	14	−2·7	24	−0·7
4	+0·3	15	+1·3	25	−3·7
5	−0·4	16	+0·3	26	+0·3
6	+2·3	17	−1·7	27	+2·7
7	−3·7	18	+1·6	28	−0·7
8	+3·8	19	+0·3	29	+0·3
9	+0·3	20	+0·3	30	+0·3
10	+2·3	21	−2·2	31	+1·0
11	+0·3				

Mean difference = +0·016 days.

Section VII. *Diurnal Tide at Portrush.*

From the data calculated from the Diurnal Tables, the solar and lunar diurnal tides at Portrush were obtained separately, as before, and found to give the following results:—

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights = 0·22 ft.
2. Maximum value of lunar tide for negative heights = 0·37 ft.
3. Maximum value of solar tide = 0·25 ft.
4. Diurnal solitidal interval = $11^h\ 80^m$.
5. Age of lunar tide = $5^d\ 9^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights = 0·15 ft.
2. Maximum value of lunar tide for negative heights = 0·19 ft.
3. Maximum value of solar tide = 0·25 ft.
4. Diurnal solitidal interval = $11^h\ 30^m$.
5. Age of lunar tide = $4^d\ 19^h$.

Adding the first two of each of the preceding, we obtain—

Range of lunar tide at high water = 0·59 ft.
Range of lunar tide at low water = 0·34 ft.

Hence by equation (3),

$$\cot(m - i_m) = \frac{0{\cdot}59}{0{\cdot}34} = \cot(29^\circ\ 57'); $$

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or, converting the arc into time,

$$m - i_m = 2^h\ 4^m;$$

but since m is the moon's hour-angle, in Portrush time, at high water, and is equal to $5^h\ 47^m$, we have, finally,

$$i_m = 3^h\ 43^m.$$

By equation (4), we have

$$\text{max. value of } 2M \sin 2\mu = \sqrt{(0{\cdot}59)^2 + (0{\cdot}34)^2} = 0{\cdot}681 \text{ ft.};$$

from which we obtain

$$M = 0{\cdot}519 \text{ ft.}$$

Also, since the maximum value of the solar tide is 0·25 feet, we have

$$\text{max. value of } 2S \sin 2\sigma = 0{\cdot}500 \text{ ft.},$$

and

$$S = 0{\cdot}342 \text{ ft.}$$

Combining these results, we have for tide constants at Portrush,—

1. Lunitidal interval $= 3^h\ 43^m$.
2. Solitidal interval $= 11^h\ 30^m$.
3. Age of lunar tide
   at high water $= 5^d\ 9^h$.
   at low water $= 4^d\ 19^h$.
4. Lunar coefficient $= 0{\cdot}519$ ft.
5. Solar coefficient $= 0{\cdot}342$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M} = 0{\cdot}659$.

The theoretical tides were constructed with the foregoing constants, and compared with the observed tides. The results of the comparison are given in the six following Tables:—

Portrush Tide, Table A.

Positive heights at high water for fifteen lunations, commencing 1850, November $7^d\ 7^h 45^m$, and ending 1851, December $22^d 5^h 35^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·35	0·39	−0·04	9	0·50	0·45	+0·05
2	0·40	0·44	−0·04	10	0·41	0·41	0·00
3	0·47	0·43	+0·04	11	0·32	0·26	+0·06
4	0·40	0·29	+0·11	12	0·30	0·24	+0·06
5	0·30	0·24	+0·06	13	0·28	0·29	−0·01
6	0·22	0·26	−0·04	14	0·30	0·38	−0·08
7	0·30	0·34	−0·04	15	0·41	0·45	−0·04
8	0·33	0·44	−0·11				
Mean difference = −0·001 ft.							

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Portrush Tide, Table B.

Negative heights at high water for fifteen lunations, commencing 1850, November $7^d\ 7^h\ 45^m$, and ending 1851, December $22^d\ 5^h\ 35^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·64	0·57	+0·07	9	0·57	0·59	−0·02
2	0·65	0·62	+0·03	10	0·52	0·57	−0·05
3	0·50	0·59	−0·09	11	0·56	0·51	+0·05
4	0·58	0·57	+0·01	12	0·70	0·42	+0·28
5	0·38	0·41	−0·03	13	0·43	0·41	+0·02
6	0·33	0·39	−0·06	14	0·42	0·51	−0·09
7	0·42	0·46	−0·04	15	0·58	0·57	+0·01
8	0·52	0·56	−0·04	16	0·55	0·57	−0·02

Mean difference = +0·002 ft.

Portrush Tide, Table C.

Positive heights at low water for fifteen lunations, commencing 1850, November $6^d\ 15^h\ 24^m$, and ending 1851, December $21^d\ 16^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·18	0·13	+0·05	9	0·17	0·28	−0·11
2	0·23	0·18	+0·05	10	0·20	0·32	−0·12
3	0·22	0·27	−0·05	11	0·25	0·26	−0·01
4	0·35	0·31	+0·04	12	0·13	0·17	−0·04
5	0·27	0·22	+0·05	13	0·14	0·07	+0·07
6	0·24	0·09	+0·15	14	0·10	0·15	−0·05
7	0·10	0·05	+0·05	15	0·18	0·25	−0·07
8	0·16	0·15	+0·01				

Mean difference = +0·001 ft.

Portrush Tide, Table D.

Negative heights at low water for fifteen lunations, commencing 1850, November $6^d\ 15^h\ 24^m$, and ending 1851, December $21^d\ 16^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·16	0·10	+0·06	9	0·18	0·27	−0·09
2	0·32	0·21	+0·11	10	0·30	0·33	−0·03
3	0·20	0·27	−0·07	11	0·28	0·32	−0·04
4	0·20	0·34	−0·14	12	0·23	0·27	−0·04
5	0·21	0·28	−0·07	13	0·10	0·11	−0·01
6	0·27	0·19	+0·08	14	0·20	0·14	+0·06
7	0·30	0·07	+0·23	15	0·16	0·19	−0·03
8	0·17	0·15	+0·02	16	0·23	0·29	−0·06

Mean difference = −0·001 ft.

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Portrush Tide, Table E.

Difference of observed and calculated times of vanishing of diurnal tide at high water, expressed in lunar days.

Age of lunar tide $=5^d\ 9^h$.

No.	Difference.	No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.		days.
1	+0·1	9	+1·8	17	−1·4	25	−0·4
2	+1·7	10	+1·5	18	+0·3	26	−0·2
3	−1·9	11	−1·7	19	−1·5	27	+0·3
4	+0·7	12	+1·8	20	+0·6	28	+3·3
5	−1·3	13	−1·4	21	−1·7	29	+0·3
6	+0·5	14	+0·9	22	−0·5	30	+3·5
7	−1·9	15	−1·4	23	−1·2	31	−1·9
8	−0·9	16	+1·3	24	+1·3		

Mean difference = +0·020 days.

Portrush Tide, Table F.

Difference of observed and calculated times of vanishing of diurnal tide at low water, expressed in lunar days.

Age of lunar tide $= 4^d\ 19^h$.

No.	Difference.	No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.		days.
1	−4·1	9	+7·3	17	+0·3	25	− 9·1
2	−4·0	10	+5·9	18	−2·9	26	+11·3
3	+0·3	11	−4·1	19	+0·4	27	+ 2·3
4	−1·1	12	+8·8	20	−0·1	28	− 6·1
5	−2·1	13	+9·3	21	−0·9	29	− 4·6
6	+0·9	14	−4·6	22	+0·3	30	− 4·7
7	+4·8	15	−4·6	23	−0·2	31	− 1·7
8	+2·3	16	−4·8	24	+5·3		

Mean difference = −0·006 days.

The agreement of the observed and calculated tides shown in Tables A, B, C, D, E is very good, but the differences in Table F are intolerable; they do not, however, vitiate the accuracy of our tidal constants for Portrush, and probably owe their origin to the fact, that the diurnal tide at low water at this station was so small in amount, that a very slight error of observation would produce a considerable error in the time of vanishing. The agreement between theory and observation, as respects the height of the diurnal tide at low water, is excellent.

Section VIII. *Diurnal Tide at Cushendall.*

The solar and lunar diurnal tides at Cushendall were deduced from the calculated tides, and found to be as follows:—

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I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights $=0{\cdot}50$ ft.
2. Maximum value of lunar tide for negative heights $=0{\cdot}33$ ft.
3. Maximum value of solar tide $=0{\cdot}30$ ft.
4. Diurnal solitidal interval $=11^h\ 25^m$.
5. Age of lunar tide $=6^d\ 18^h\ 41^m$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights $=0{\cdot}355$ ft.
2. Maximum value of lunar tide for negative heights $=0{\cdot}45$ ft.
3. Maximum value of solar tide $=0{\cdot}25$ ft.
4. Diurnal solitidal interval $=11^h\ 25^m$.
5. Age of lunar tide $=5^d\ 2^h\ 45^m$.

Adding the first two of each of the preceding results, we find—

Range of lunar tide at high water $=0{\cdot}83$ ft.
Range of lunar tide at low water $=0{\cdot}805$ ft.

Hence by equation (3),

$$\cot(m-i_m)=\frac{0{\cdot}830}{0{\cdot}805}=\cot(44^\circ\ 7');$$

or, converting the arc into time,

$$m-i_m=3^h\ 2^m;$$

but since m, the moon's hour-angle at high water expressed in Cushendall time, is $10^h\ 18^m$, we obtain

$$i_m=7^h\ 16^m.$$

By equation (4), we have

$$\text{max. value of } 2M\sin 2\mu=\sqrt{(0{\cdot}83)^2+(0{\cdot}805)^2}=1{\cdot}156 \text{ ft};$$

from which we obtain

$$M=0{\cdot}881 \text{ ft.}$$

Also, since the mean value of the solar tide is 0·275 feet, we have

$$\text{max. value of } 2S\sin 2\sigma=0{\cdot}550 \text{ ft.},$$

and

$$S=0{\cdot}376 \text{ ft.}$$

Combining the foregoing results, we find for the tide constants at Cushendall,—

1. Lunitidal interval $=7^h\ 16^m$.
2. Solitidal interval $=11^h\ 25^m$.
3. Age of lunar tide
   at high water $=6^d\ 18^h\ 41^m$.
   at low water $=5^d\ 2^h\ 45^m$.
4. Lunar coefficient $=0{\cdot}881$ ft.
5. Solar coefficient $=0{\cdot}376$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M}=0{\cdot}427$.

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The theoretical tides were constructed with the foregoing constants, and compared with the observed tides; the results of this comparison are contained in the following Tables.

Cushendall Tide, Table A.

Positive heights at high water for thirteen lunations, commencing 1851, January $2^d 22^h 33^m$, and ending 1851, December $23^d 9^h 54^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·70	0·64	+0·06	8	0·70	0·70	0·00
2	0·73	0·70	+0·03	9	0·66	0·68	−0·02
3	0·54	0·52	+0·02	10	0·48	0·52	−0·04
4	0·40	0·34	+0·06	11	0·45	0·43	+0·02
5	0·40	0·40	0·00	12	0·51	0·51	0·00
6	0·50	0·50	0·00	13	0·58	0·60	−0·02
7	0·52	0·63	−0·11				

Mean difference = 0·000 ft.

Cushendall Tide, Table B.

Negative heights at high water for thirteen and a half lunations, commencing 1851, January $2^d 22^h 33^m$, and ending 1852, January $6^d 2^h 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·56	0·50	+0·06	8	0·59	0·58	+0·01
2	0·50	0·55	−0·05	9	0·50	0·53	−0·03
3	0·36	0·44	−0·08	10	0·38	0·37	+0·01
4	0·20	0·23	−0·03	11	0·20	0·23	−0·03
5	0·20	0·28	−0·08	12	0·33	0·29	+0·04
6	0·40	0·35	+0·05	13	0·53	0·43	+0·10
7	0·47	0·43	+0·04	14	0·55	0·58	−0·03

Mean difference = −0·001 ft.

Cushendall Tide, Table C.

Positive heights at low water for thirteen lunations, commencing 1851, January $0^d 14^h 15^m$, and ending 1851, December $22^d 3^h 33^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·62	0·52	+0·10	8	0·40	0·49	−0·09
2	0·31	0·39	−0·08	9	0·28	0·40	−0·12
3	0·30	0·33	−0·03	10	0·40	0·36	+0·04
4	0·40	0·36	+0·04	11	0·57	0·43	+0·14
5	0·56	0·52	+0·04	12	0·54	0·50	+0·04
6	0·45	0·54	−0·09	13	0·61	0·57	+0·04
7	0·52	0·57	−0·05				

Mean difference = −0·001 ft.

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Cushendall Tide, Table D.

Negative heights at low water for thirteen lunations, commencing 1851, January $0^d\,14^h\,15^m$, and ending 1851, December $22^d\,3^h\,33^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·54	0·52	+0·02	8	0·65	0·54	+0·11
2	0·57	0·51	+0·06	9	0·50	0·47	+0·03
3	0·37	0·40	−0·03	10	0·47	0·49	−0·02
4	0·52	0·53	−0·01	11	0·57	0·54	+0·03
5	0·54	0·59	−0·05	12	0·60	0·64	−0·04
6	0·60	0·69	−0·09	13	0·72	0·69	+0·03
7	0·60	0·67	−0·07				

Mean difference = −0·002 ft.

Cushendall Tide, Table E.

Difference of observed and calculated times of vanishing of diurnal tide at high water, expressed in lunar days.

Age of lunar tide = $6^d\,18^h\,41^m$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+1·32	10	+0·52	19	−0·58
2	−0·08	11	−0·08	20	−0·08
3	−0·08	12	+0·52	21	−0·68
4	+0·22	13	−1·58	22	−0·88
5	+1·22	14	+1·42	23	−0·58
6	+1·22	15	−0·08	24	−0·08
7	−1·58	16	+0·42	25	−0·08
8	+0·92	17	−0·68	26	−0·08
9	−0·78,	18	+0·32		

Mean difference = +0·004 days.

Cushendall Tide, Table F.

Difference of observed and calculated times of vanishing of diurnal tide at low water, expressed in lunar days.

Age of lunar tide = $5^d\,2^h\,45^m$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·55	10	+0·05	19	+1·55
2	+0·55	11	−0·05	20	+1·85
3	+1·55	12	−0·45	21	−1·25
4	+0·55	13	+0·55	22	+0·55
5	−0·85	14	−0·45	23	+0·55
6	−0·45	15	+1·05	24	+0·55
7	−1·05	16	−0·75	25	+0·55
8	−0·65	17	+0·55	26	−1·65
9	−1·85	18	−1·05		

Mean difference = 0·000 days.

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From the preceding Tables, it is evident that the utmost reliance may be placed in the values of the tide constants at this station.

Section IX. *Diurnal Tide at Donaghadee.*

From the Diurnal Tables, the solar and lunar diurnal tides at Donaghadee were calculated separately, and found to give the following results:—

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights = 0·42 ft.
2. Maximum value of lunar tide for negative heights = 0·38 ft.
3. Maximum value of solar tide = 0·28 ft.
4. Diurnal solitidal interval = $11^h\ 12^m$.
5. Age of lunar tide = $6^d\ 5^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights = 0·39 ft.
2. Maximum value of lunar tide for negative heights = 0·42 ft.
3. Maximum value of solar tide = 0·28 ft.
4. Diurnal solitidal interval = $11^h\ 12^m$.
5. Age of lunar tide = $5^d\ 2^h$.

Adding the first two of each of the preceding, we obtain,—

Range of lunar tide at high water = 0·80 ft.
Range of lunar tide at low water = 0·81 ft.

Hence by equation (3),

$$\cot(m - i_m) = \frac{0{\cdot}80}{0{\cdot}81} = \cot(45^\circ\ 21'),$$

or, converting the arc into time,

$$m - i_m = 3^h\ 7^m;$$

but since m is the moon's hour-angle in Donaghadee time, at high water, and is $10^h\ 40^m$, we obtain, finally,

$$i_m = 7^h\ 33^m.$$

By equation (4), we have

$$\text{max. value of } 2M \sin 2\mu = \sqrt{(0{\cdot}80)^2 + (0{\cdot}81)^2} = 1{\cdot}139 \text{ ft.};$$

from which we obtain

$$M = 0{\cdot}868 \text{ ft.}$$

Also, since the maximum value of the solar tide is 0·28 ft., we have, by equation 5,

$$\text{max. value of } 2S \sin 2\sigma = 0{\cdot}56 \text{ ft.},$$

and

$$S = 0{\cdot}383 \text{ ft.}$$

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Combining these results, we have for the tide constants at Donaghadee,—

1. Lunitidal interval $=7^h\ 33^m$.
2. Solitidal interval $=11^h\ 12^m$.
3. Age of lunar tide
   at high water $=6^d\ 5^h$.
   at low water $=5^d\ 2^h$.
4. Lunar coefficient $=0{\cdot}868$ ft.
5. Solar coefficient $=0{\cdot}383$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M}=0{\cdot}441$.

The theoretical tides at Donaghadee were constructed with the foregoing tide constants, and compared with the observed tides, with the following results.

Donaghadee Tide, Table A.

Positive heights at high water for sixteen and a half lunations, commencing 1850, September $29^d\ 5^h\ 51^m$, and ending 1851, December $22^d\ 22^h\ 24^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·40	0·37	+0·03	10	0·41	0·45	−0·04
2	0·26	0·30	−0·04	11	0·60	0·52	+0·08
3	0·43	0·40	+0·03	12	0·60	0·65	−0·05
4	0·47	0·45	+0 02	13	0·58	0·59	−0·01
5	0·57	0·59	−0·02	14	0·40	0·42	−0·02
6	0·63	0·59	+0·04	15	0·37	0·38	−0·01
7	0·50	0·49	+0·01	16	0·50	0·43	+0·07
8	0·30	0·32	−0·02	17	0·46	0·52	−0·06
9	0·33	0·35	−0·02				

Mean difference $=-0{\cdot}000$ ft.

Donaghadee Tide, Table B.

Negative heights at high water for sixteen lunations, commencing 1850, September $29^d\ 5^h\ 51^m$, and ending 1852, January $5^d\ 21^h\ 12^m$.

No.	Obse rved.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·39	0·28	+0·11	10	0·48	0·48	0·00
2	0·40	0·31	+0·09	11	0·60	0·58	+0·02
3	0·45	0·46	−0·01	12	0·50	0·56	−0·06
4	0·59	0·54	+0·05	13	0·47	0·48	−0·01
5	0·60	0·58	+0·02	14	0·32	0·33	−0·01
6	0·44	0·48	−0·04	15	0·34	0·29	+0·05
7	0·24	0·37	−0·13	16	0·46	0·38	+0·08
8	0·24	0·31	−0·07	17	0·56	0·55	+0·01
9	0·34	0·40	−0·06				

Mean difference $=+0{\cdot}002$ ft.

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Donaghadee Tide, Table C.

Positive heights at low water for sixteen lunations, commencing 1850, October $12^d\ 4^h$, and ending 1851, December $22^d\ 16^h\ 27^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·60	0·46	+0·14	9	0·44	0·59	−0·15
2	0·62	0·57	+0·05	10	0·55	0·64	−0·09
3	0·61	0·62	−0·01	11	0·48	0·57	−0·09
4	0·70	0·62	+0·08	12	0·36	0·52	−0·16
5	0·35	0·43	−0·08	13	0·46	0·39	+0·07
6	0·26	0·43	−0·17	14	0·60	0·42	+0·18
7	0·42	0·41	+0·01	15	0·59	0·51	+0·08
8	0·57	0·50	+0·07	16	0·65	0·61	+0·04

Mean difference = −0·002 ft.

Donaghadee Tide, Table D.

Negative heights at low water for sixteen lunations, commencing 1850, October $12^d\ 4^h$, and ending 1851, December $22^d\ 16^h 27^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·46	0·55	−0·09	9	0·66	0·68	−0·02
2	0·66	0·66	0·00	10	0·60	0·64	−0·04
3	0·55	0·67	−0·12	11	0·60	0·52	+0·08
4	0·60	0·54	+0·06	12	0·45	0·47	−0·02
5	0·51	0·52	−0·01	13	0·40	0·42	−0·02
6	0·35	0·42	−0·07	14	0·60	0·47	+0·13
7	0·49	0·46	+0·03	15	0·72	0·62	+0·10
8	0·59	0·57	+0·02	16	0·74	0·67	+0·07

Mean difference = −0·006 ft.

Donaghadee Tide, Table E.

Difference of observed and calculated times of vanishing of diurnal tides at high water, expressed in lunar days.

Age of lunar tide $= 6^d\ 5^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	−0·65	12	+1·05	23	+0·15
2	−1·25	13	+1·65	24	−2·00
3	−3·65	14	−1·20	25	+0·25
4	−2·35	15	+0·65	26	+0·65
5	+1·20	16	−1·10	27	−1·35
6	0·00	17	+1·75	28	−0·05
7	+4·05	18	−0·35	29	−1·05
8	+0·30	19	+0·75	30	−0·35
9	+0·95	20	−1·45	31	+0·55
10	+0·15	21	+0·60	32	−0·60
11	+1·15	22	−0·25	33	+1·90

Mean difference = +0·003 days.

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Donaghadee Tide, Table F.

Difference of observed and calculated times of vanishing of diurnal tides at low water, expressed in lunar days.

Age of lunar tide $=5^d\ 2^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·10	12	−1·10	23	−0·35
2	+1·00	13	−0·95	24	+0·85
3	−0·45	14	−0·05	25	−0·20
4	+1·00	15	−0·40	26	+1·75
5	+0·20	16	−0·90	27	+0·75
6	−2·80	17	−0·10	28	−1·05
7	−0·65	18	+0·20	29	+1·60
8	+1·30	19	−0·55	30	+1·30
9	+0·85	20	+1·15	31	+0·75
10	+1·10	21	−0·65	32	−1·35
11	−1·45	22	+1·45	33	−2·00

Mean difference $=+0{\cdot}010$ days.

The agreement of the observed and calculated diurnal tides shown in the preceding Tables is excellent.

[To be continued.]

XV. *On the relation of Diamagnetic Polarity to Magnecrystallic Action.* By JOHN TYNDALL, *F.R.S. &c.**

IN a communication presented to the Royal Society some weeks ago, the fact of diamagnetic polarity was established in the case of insulators, among which phosphorus, sulphur, calcareous spar, statuary marble, heavy glass, nitre, and wax were comprised. The demonstration was also extended to distilled water and other liquids; and thus the conditions proposed by the opponents of diamagnetic polarity for its rigorous demonstration were fulfilled. The importance of the principle is demonstrated by the fruitfulness of its consequences; for by it we obtain a clear insight of phænomena which, without it, would remain standing enigmas in science, being connected by no known tie with the ordinary laws of mechanics. Many of the phænomena of magnecrystallic action are of this paradoxical character. For the sake of those who see no clear connexion between these phænomena and the other effects of magnetism, as well as for the sake of completeness, I will here endeavour to indicate in a simple manner, and from my own point of view, the bearing of the question of polarity upon that of magnecrystallic action. I will commence with the elementary phænomena, and select for illustration as I proceed,

* Communicated by the Author.

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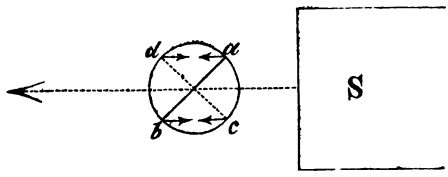
cases of real difficulty which have been actually encountered by those who have worked experimentally at the subject.

To liberate the thoughts from all effects except those which are purely magnecrystallic, we will for the present operate with spheres. Let a sphere of carbonate of lime be suspended before the pole S, fig. 1, of an electro-magnet, so that the axis of the crystal shall be horizontal. Let the line *ab* mark any position of the axis inclined to the direction of the force emanating from S; and let the dotted line *dc* make an equal angle with the direction of the force at the other side. As the sphere is diamagnetic, the face of it which is turned towards S will be hostile to S, while that turned *from* S will be friendly to S, according to the principles established in the paper above referred to; and, if the sphere were homogeneous, the tendency to set *ab* at right angles to the direction of the force would be exactly neutralized by the tendency to set *cd* in the same position: the sphere would consequently stand still. But the case is otherwise if the intensity of diamagnetization along *ab* be greater than along *cd*, which I have elsewhere shown to be the fact*. If we suppose the sphere to vanish, with the exception of two thin needles taken along the lines mentioned, the hostile pole at *a* will be stronger than that at *c*, and the friendly pole at *b* will be stronger than that at *d*; hence the ends *a* and *b* being acted upon by a mechanical couple of superior power, the line *ab* will recede from its inclined position, and finally set itself *at right angles* to the direction of the force. Whatever be the inclination of the line *ab* to the magnetic axis, this superiority will belong to its couple; it is therefore manifest that the entire sphere will turn in the manner here indicated, and finally set with the axis of the crystal equatorial, which is the result established by experiment.

Fig. 1.



For the diamagnetic calcium, contained in this crystal, let the magnetic element, iron, be substituted. Each molecule of the crystal becomes thereby magnetic; we have carbonate of iron in place of carbonate of lime; and the line which, in the latter substance is that of maximum repulsion, is that of maximum attraction in the former. This, I think, is one of the most suggestive points†

* Phil. Mag. S. 4. vol. ii. p. 176.

† For its bearing upon the question of a magnetic medium see Phil. Mag. vol. ix. p. 208.

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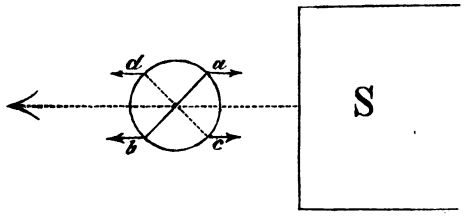
that researches in magnecrystallic action have established, namely, that the self-same arrangement of particles influences the paramagnetic and diamagnetic forces in the self-same way, intensifying both in the same direction. Let us suppose, then, that the sphere of carbonate of iron is suspended as in fig. 2, the line *ab* being the axis of the crystal. I have already shown that this line is that in which the magnetic induction is most intense*. Comparing, as before, the lines *ab* and *cd*, the friendly pole *a* is stronger than *c*, and the hostile pole *b* is stronger than *d*; a residual "couple" therefore acts upon *ab* in the direction indicated by the arrows, which will finally set this line *parallel* to the direction of the force. This is also the result which experiment exhibits.

Fig. 2.



We will now proceed to apply the principle of polarity to some of the more complicated forms of magnecrystallic action. Some highly paradoxical effects were adduced by Mr. Faraday as illustrative of his earlier impressions regarding this question, and I cannot bring the subject in a clearer manner before the reader than by quoting Mr. Faraday's own description of the phænomena referred to. Here it follows:—

"Another very striking series of proofs that the effect is not due to attraction or repulsion was obtained in the following manner. A skein of fifteen filaments of cocoon silk, about 14 inches long, was made fast above, and then a weight of an ounce or more hung to the lower end; the middle of this skein was about the middle of the magnetic field of the electro-magnet, and the square weight below rested against the side of a block of wood so as to give a steady silken vertical axis without swing or revolution. A small strip of card, about half an inch long and the tenth of an inch broad, was fastened across the middle of this axis by cement; and then a small prismatic crystal of sulphate of iron 0·3 of an inch long and 0·1 in thickness, was attached to the card, so that the length and also the magnecrystallic axis were in the horizontal plane; all the length was on one side of the silken axes, so that as the crystal swung round, the length was radius to the circle described, and the magnecrystallic axis parallel to the tangent.

"When the crystal was made to stand between the flat-faced poles, the moment the magnet was excited it moved, tending to

* Phil. Mag. S. 4. vol. ii. p. 177.

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stand with its length equatorial, or its magnecrystallic axis parallel to the lines of force. When one pole was removed and the experiment repeated, the same effect took place, but not so strongly as before; finally, when the pole was brought as near to the crystal as it could be without touching it, the same result occurred, and with more strength than in the last case. In the two latter experiments, therefore, the crystal of sulphate of iron, though a magnetic body, and strongly attracted by such a magnet as that used, actually *receded* from the pole of the magnet under the influence of the magnecrystallic condition.

"If the pole S* be removed and that marked N be retained for action on the crystal, then the latter approaches the pole urged by both the magnetic and magnecrystallic forces; but if the crystal be revolved 90° to the left, or 180° to the right, round the silken axis, so as to come into the contrary or opposite position, then this pole repels or rather causes the removal to a distance of the crystal, just as the former did. The experiment requires care, and I find that conical poles are not good; but with attention I could obtain the results with the utmost readiness.

"The sulphate of iron was then replaced by a crystalline plate of bismuth, placed, as before, on one side of the silk suspender, and with its magnecrystallic axis horizontal†. Making the position the same as that which the crystal had in relation to the N pole in the former experiment, so that to place its axis parallel to the lines of magnetic force it must approach this magnetic pole, and then throwing the magnet into an active state, the bismuth moved accordingly and did approach the pole, against its diamagnetic tendency, but under the influence of the magnecrystallic force.

"Hence a proof that neither attraction nor repulsion governs the set. . . . . This force, then, is distinct in its character and effects from the magnetic and diamagnetic forms of force."

These experiments present grave mechanical difficulties, and are quite sufficient to justify the conclusion drawn from them, namely, that the force which produces them is *neither* attractive *nor* repulsive. We will now endeavour to apply the idea of a force which is *both* attractive *and* repulsive, or in other words of a *polar* force, to the solution of the difficulty.

* The figures will be given and explained further on.

† It will be borne in mind that Mr. Faraday calls the line in a crystal which sets from pole to pole, the magnecrystallic axis of the crystal, whether the latter is paramagnetic or diamagnetic. In bodies of the former class, however, the "axis" sets from pole to pole because the attraction along it is a maximum; while in bodies of the latter class, the "axis" sets from pole to pole because the repulsion along the line perpendicular to it is a maximum.

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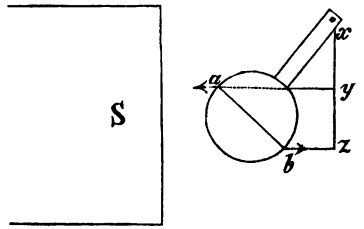
For the sake of disencumbering the mind of all considerations save those which belong to pure magnecrystallic action, we will suppose, as before, the bodies experimented with to be spherical.

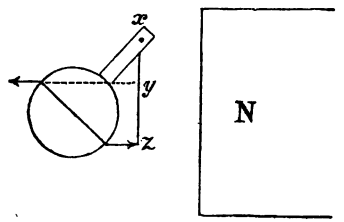
Let the dot at x, fig. 3, be the intersection of the silken axes with Mr. Faraday's strip of card; and on the end of the strip, let the sphere of sulphate of iron be placed with its magnecrystallic axis ab at right angles to the length of the strip. This line, as I have already shown*, is that of most intense magnetization through the crystals. The forces acting on the sphere in its present position are exactly similar to those acting upon the carbonate of iron in fig. 2. A residual "couple" will apply itself at the extremities of ab, as indicated by the arrows, and would, if the sphere were free to turn round its centre of gravity, set the line ab parallel to the magnetic axis. But the sphere is here rigidly connected with a lever moveable round its own axis of suspension, and it is easy to state the mechanical result that must follow from this arrangement. To obtain the moments of the two forces acting upon a and b, we have to multiply each of them by its distance from the axis x. In front of a flat pole such as that made use of by Mr. Faraday in these experiments, the force diminishes very slowly as we recede from the pole, so that the attraction of a does not so far exceed the repulsion of b as to prevent the product of the latter into xz from exceeding that of the former into xy, and consequently the paramagnetic sphere must recede from the pole.

Fig. 3.



In his next experiment, Mr. Faraday removed the pole S and allowed the pole N to act upon the crystal as in fig. 4. In this case it will be seen that the end nearest the pole, and therefore the most strongly attracted, is also at the greatest distance from the axis of rotation. Hence the sphere must approach the pole, as it does in the experiment.

Fig. 4.



When the strip of card is revolved 90°, we have the state of things shown in fig. 5; and when it is revolved 180°, we have

* Phil. Mag. S. 4. vol. ii. p. 178.

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the state of things shown in fig. 6. It is manifest, for the

Fig. 5.

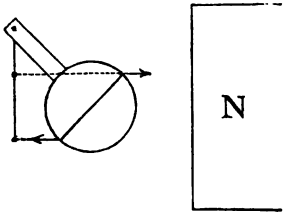
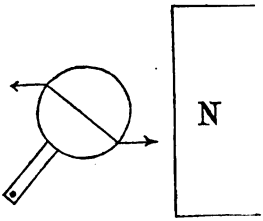


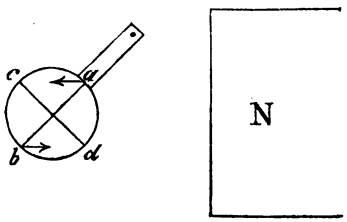
Fig. 6.



mechanical reasons already assigned, that the crystal, in both these cases, must recede from the pole.

Substituting for the sphere of sulphate of iron a sphere of bismuth with its magnecrystallic axis *cd*, fig. 7, perpendicular to the strip of card, the bismuth is found to approach the pole when the magnet is excited. The line *ab* perpendicular to that named the magnecrystallic axis, has been shown by Mr. Faraday to be that of greatest diamagnetic intensity; the mass is therefore under the influence of forces precisely similar to those acting on the carbonate of lime in fig. 1. A residual couple, as denoted by the arrows, will act at the extremities of the line *ab*. The absolute repulsion of *a* in the field of force here assumed, does not differ much from the absolute attraction of *b*; but the latter force acts at the end of a much longer lever, and consequently the sphere is drawn towards the excited pole. I cannot help remarking here upon the severe faithfulness with which these results are recorded, and on the inestimable value of such records to scientific progress. The key to their solution being once found, the investigator may proceed confidently to the application of his principles, without fear of check or perplexity arising from the imperfection of his data.

Fig. 7.



In all these cases we have assumed that the magnetic force diminishes slowly as we recede from the pole, for this is essential to the production of the effects. The exact expression of the condition is, that the advantage due to the proximity of the part of the mass nearest the pole, must be less than that arising from the

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greater leverage possessed by the force acting on the more distant parts. When the shape of the poles is such that the diminution of the force with the increase of distance is too speedy for the above condition to be fulfilled, the phænomena no longer exhibit themselves. It is plain that the diminution of the force as we recede from a pointed pole must be more speedy than when we recede from a magnetized surface, and hence it is that Mr. Faraday finds that "conical poles are not good." It is also essential that the length of the lever which supports the magnecrystallic body shall bear a sensible ratio to the distance between the two points of application of the magnetic force. If the lever be long, *recession* will take place in cases where, with a shorter lever, *approach* would be observed.

It is well known that a piece of soft iron is attracted most strongly by the angles and corners of a magnet, and hence it is inferred that the magnetic force emanating from these edges and corners is more intense than that issuing from the central parts of the polar surfaces. Such experiments, however, when narrowly criticised, do not justify the inference drawn from them. They simply show that the difference between attraction and repulsion, on which the final attraction depends, is greater at the edges than elsewhere; but they do not enable us to infer the absolute strength of either the attraction or the repulsion, or in other words, of the force of magnetization. The fact really is, that while the attraction of the mass is nearly absent in the central portion of a magnetic field bounded by two flat poles, the magnetization is really stronger there than between the edges. This is proved by the following experiment:—I suspended a cube of crystallized bismuth from a fibre of cocoon silk, and when the magnet was excited, the cube set its planes of principal cleavage equatorial. When drawn aside from this position and liberated, it oscillated round it. Between the upper edges of the moveable poles the number of oscillations performed in a minute was seventy-six; in the centre of the field the number performed was eighty-eight, and between the lower edges eighty. A cube of magnetic slate, similarly suspended, oscillated in the centre of the field forty-nine times, and between the edges only forty times, in fifteen seconds. In the former position there was no sensible tendency of the cube to move towards either pole; but in the latter position, though the magnetization was considerably less intense, the cube was with difficulty prevented from moving up to one or the other of the edges. The reason of all this manifestly is, that while the forces in the centre of the field nearly neutralize each other as regards the *translation* of the mass, they are effective in producing its *oscillation*; while between the edges, though the absolute forces acting on the north and south poles of the

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excited substances are less intense, the *difference* of these forces, owing to the speedier diminution of the force with the distance, is greater than in the centre of the field. It is therefore an error to infer, that, because the attraction of the mass is greater at the edges and corners than in the centre of the field, the magnetizing force of the former must therefore be more intense than that of the latter*.

There is another interesting and delicate experiment of Mr. Faraday's to which I am anxious to apply the principle of diamagnetic polarity: the experiment was made with a view of proving that "the magnecrystallic force is a force acting at a distance." "The crystal," writes Mr. Faraday, "is moved by the magnet at a distance, and the crystal can also move the magnet at a distance. To produce the latter result, I converted a steel bodkin, 3 inches long, into a magnet, and then suspended it vertically by a cocoon filament from a small horizontal rod, which again was suspended by its centre and another length of cocoon filament, from a fixed point of support. In this manner the bodkin was free to move on its own axis, and could also describe a circle about $1\frac{1}{2}$ inch in diameter; and the latter motion was not hindered by any tendency of the needle to point under the earth's influence, because it could take any position in the circle and yet remain parallel to itself.

"When a crystal of bismuth was fixed on a support with the magnecrystallic axis in a horizontal direction, it could be placed near the lower pole of the magnet in any position; and being then left for two or three hours, or until by repeated examination the magnetic pole was found to be stationary, the place of the latter could be examined, and the degree and direction in which it was affected by the bismuth ascertained. . . . . The effect produced was small; but the result was, that if the direction of the magnecrystallic axis made an angle of 10°, 20°, or 30° with the line from the magnetic pole to the middle of the bismuth crystal, then the pole followed it, tending to bring the two lines into parallelism; and this it did whichever end of the magnecrystallic axis was towards the pole, or whichever side it was inclined to. By moving the bismuth at successive times, the deviation of the magnetic pole could be carried up to 60°. The crystal, therefore, is able to react upon the magnet at a distance. But though it thus takes up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation; which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances."

* Some important consequences resulting from this experiment are intended for a future communication.

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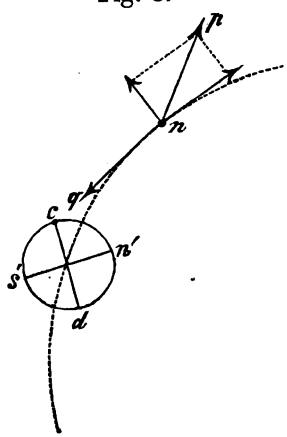
The disposition of this important experiment will be manifest from fig. 8, where *cd* is the magnecrystallic axis of a sphere of bismuth, or the line in which the diamagnetic induction is least intense; and $s'n'$ the direction of the principal cleavage, or that of most intense diamagnetization. Let *n* be the point of the bodkin, say its north pole, the crystal will be excited by the influence of this pole, and the resultant action will be the same as if it were exclusively "diamagnetized" along the line $s'n'$. At the end nearest to the pole of the bodkin a repelled pole n' will be excited in the bismuth; at the most distant end an attracted pole s' will be excited. Let the repulsive force tending to separate *n* from n' be represented by the line *np*, and let the attraction exerted between s' and *n* be represented by the line *nq*; the arrangement is such that the force of s' acts more nearly in the direction of the tangent than that of n'; the latter may be decomposed into two, one acting along the circle and the other across it: the latter component exerts a pressure against the axis of suspension; the former only is effective in causing the pole *n* to move; so that the whole, or nearly the whole, of the attraction has to compete with a comparatively small component of the repulsion. The former therefore preponderates, and the pole *n* approaches the crystal. It is manifest that as the angle which the line from *n* to the centre of the crystal makes with the magnecrystallic axis, increases, the component of repulsion which acts in the direction of a tangent to the curve, augments also; and that at a certain point this component must become preponderant. Beyond an angle of 30° it is to be presumed that Mr. Faraday did not obtain the effect. Removing the crystal, and placing a small magnet in the position of the line $s'n'$, with its poles arranged as in the figure, the same phænomena would be produced*.

Fig. 8.



As finally illustrative of the sufficiency of the principle of polarity to explain the most complicated phænomena of magnecrystallic action, let us turn to the consideration of those curious effects of rotation first observed by M. Plücker, and which I

* As there are no measurements given of the distances between the crystal and the pole, it is of course impossible to do more than indicate generally the theoretic solution of the experiment.

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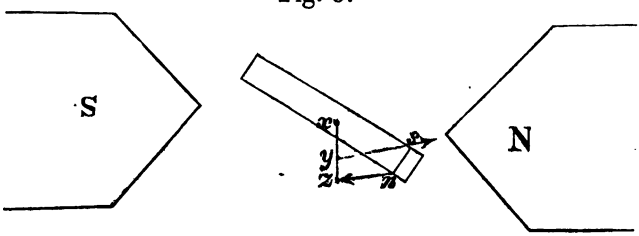
have illustrated by thirty-seven cases in the Bakerian Lecture for 1855. The effects, it will be remembered, consisted of the turning of elongated paramagnetic bodies suspended between *pointed poles* from the axial to the equatorial position, and of elongated diamagnetic bodies, from the equatorial to the axial position, when the distance between the suspended body and the influencing poles was augmented. I know this to be a subject of considerable difficulty to many, and I therefore claim the indulgence of those who have paid more than ordinary attention to it, if in my explanation I should appear to presume too far on the reader's want of acquaintance with the question. Let us then suppose an elongated crystal of tourmaline, staurolite, ferrocyanide of potassium, or beryl, to be suspended between the conical poles N, S, fig. 9, of an electro-magnet; supposing the position between the poles to be the oblique one shown in the figure, let us inquire what are the forces acting

Fig. 9.



upon the crystal in this position. In the case of all paramagnetic crystals which exhibit the phænomenon of rotation, it will be borne in mind that the line of most intense magnetization is at right angles to the length of the crystal. Let *sn* be any transverse line near the end of the crystal; fixing our attention for the present on the action of the pole N, we find that a friendly pole is excited at *s* and a hostile pole at *n*: let us suppose *s* and *n* to be the points of application of the polar force, and, for the sake of simplicity, let us assume the distances from the point of the pole N to *s* and from *s* to *n*, to be equal to one another. We will further suppose the action of the pole to be that of a magnetic point, to which, in reality, it approximates; then, inasmuch as the quantities of north and south magnetism are equal, we have simply to apply the law of inverse squares to find the difference between the two forces. Calling that acting on *s* unity, that acting on *n* will be $\frac{1}{4}$. Opposed to this difference of the absolute forces is the difference of their moments of rotation; the force acting on *n* is applied at a greater distance from the axis of rotation, but it is manifest that to counterbalance the advantage enjoyed by *s*, on account

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of its greater proximity, the distance $x\,z$ would require to be four times that of $x\,y$. Taking the figure as the correct sketch of the poles and crystal, it is plain that this condition is not fulfilled, and that hence the end of the crystal will be drawn towards the pole N. What we have said of the pole N is equally applicable to the pole S, so that such a crystal suspended between two such poles, in the manner here indicated, will set its length along the line which unites them.

While the crystal retains the position which it occupied in fig. 9, let the poles be removed further apart, say to ten times their former distance. The ratio of the two forces acting on the two points of application s and n will be now as the square of 11 to the square of 10, or as 6 : 5 nearly. Taking fig. 10, as in the former case, to be the exact sketch of the crystal, it is

Fig. 10.

manifest that the ratio of $x\,z$ to $x\,y$ is greater than that of 6 to 5; the advantage, on account of greater leverage, possessed by the force acting on n is therefore greater than that which greater proximity gives to s, and the consequence is that the crystal will recede from the pole, and its position of rest between two poles placed at this distance apart will be at right angles to the line which joins them. It is needless for me to go over the reasoning in the case of a diamagnetic body whose line of strongest diamagnetization is perpendicular to its length. Reversing the direction of the arrows in the last two figures, we should have the graphic representation of the forces acting upon such a body; and a precisely analogous mode of reasoning would lead us to the conclusion, that when the polar points are near the crystal, the latter will be driven towards the equatorial position, while where they are distant, the crystal will be drawn into the axial position. In this way the law of action laid down empirically in the Bakerian Lecture for 1855 is deduced *à priori* from the polar character of both the magnetic and diamagnetic forces. The most complicated effects of magnecrystallic action are thus reduced to mechanical problems of extreme simplicity; and, inasmuch as these actions are perfectly inexplicable except on the assumption of diamagnetic polarity, they add their evidence

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in favour of this polarity to that already furnished in such abundance.

Perhaps as remarkable an illustration as could be chosen of the apparently perplexing character of certain magnetic phænomena, but of their real simplicity when the exact nature of the force producing them is understood, is furnished by the following experiment. I took a quantity of pure bismuth powder and squeezed it between two clean copper plates until the powder became a compact mass. A fragment of the mass suspended before the pointed pole of a magnet was forcibly repelled; and when suspended in the magnetic field with the direction of pressure horizontal, in accordance with results already sufficiently well known, it set its line of pressure equatorial.

A second quantity of the bismuth powder was taken, and with it was mixed a quantity of powdered carbonate of iron, amounting to $\frac{3}{10}$ths per cent. of the whole; the mass was still strongly diamagnetic, but the line of compression, instead of setting equatorial, as in the former instance, set decidedly axial.

A quantity of the mixed powder was next taken, in which the magnetic constituents amounted to 1 per cent. The mass was still diamagnetic, but the line of compression set axial; it did so when the influence of exterior form was quite neutralized, so that the effect must be referred solely to the compression of the mass. With 2 per cent. of carbonate of iron powder the mass was magnetic, and set with increased energy its line of compression axial; with 4 per cent. of carbonate of iron the same effect was produced in a still more exalted degree.

Now, why should the addition of a quantity of carbonate of iron powder, which is altogether insufficient to convert the mass from a diamagnetic to a paramagnetic one, be able to overturn the tendency of the diamagnetic body to set its line of compression equatorial? The question is puzzling at first sight, but the difficulty vanishes on reflection. The repulsion of the mass of bismuth, suspended before a pointed pole, depends upon the general capacity of the mass for diamagnetic induction, while its position as a magnecrystal between the flat poles depends on the difference between its capacities in two different directions. The diamagnetic capacity of the mass may be very great, while its capacity in different directions may be nearly alike, or quite so: the former, in the case before us, came into play before the pointed pole; but between the flat poles, where the *directive*, and not the *translative* energy is great, the carbonate of iron powder, whose directive power, when compressed, far exceeds that of bismuth, determined the position of the body. In this simple way a number of perplexing results obtained with bodies formed of a mixture of paramagnetic and

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diamagnetic constituents, is capable of satisfactory explanation.

Finally, inasmuch as the set of the mass in the magnetic field depends upon the difference of its excitement in different directions, it will follow that any circumstance which affects all directions of a magnecrystallic mass in the same degree will not disturb the differential action upon which its deportment depends. This seems to me to be the explanation of the results recently obtained by Mr. Faraday with such remarkable uniformity, namely, that, no matter what the medium may be in which the magnecrystallic body is immersed, whether air or liquid, paramagnetic or diamagnetic, it requires, in all cases, the same amount of force to turn it from the position which it takes up in virtue of its structure.

I have thus dwelt upon instances of magnecrystallic action which have revealed themselves in actual practice, as affording the best examples for the application of the knowledge which the demonstration of the polarity of the diamagnetic force places in our possession; and I believe it has been shown that these phænomena, which were in the highest degree paradoxical when first announced, are deducible with as much ease and certainty from the action of polar forces, as the precession of the equinoxes is from the force of gravitation. The whole domain of magnecrystallic action is thus transferred from a region of mechanical enigmas to one in which our knowledge is as clear and sure as it is regarding the most elementary phænomena of magnetic action.

Royal Institution, Dec. 1855.

XVI. *On Ozone and Ozonic Actions in Mushrooms.* By M. Schönbein*.

My dear Faraday,

.... YOU know that I hold oxygen, both in its free and bound state, to be capable of existing in two allotropic modifications,—in the ozonic or active, and the ordinary or inactive condition. All the oxy-compounds yielding common oxygen at a raised temperature I consider to contain ozonized oxygen; and I am further inclined to believe that the disengagement of common oxygen from those compounds depends upon the transformation of the ozonized oxygen into the inactive one, or, to denote that allotropic change, of $\dot{O}$ into O. Now a general fact is this: that the oxygen thus set free always contains traces of $\dot{O}$

* Communicated by Professor Faraday.

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more or less, according to the degree of temperature at which the oxygen happens to be disengaged from those compounds. The lower that degree, the larger the quantity of Ô mixed with O; though I must not omit to state, that in all cases that quantity happens to be exceedingly small in comparison to that of O obtained at the same time. The best means of ascertaining the presence of Ô is the alcoholic solution of guaiacum recently prepared. You know that O does not in the least change the colour of that resiniferous liquid, whilst free Ô or PbO + Ô, &c. have the power of colouring it deep blue. The blue matter is, as I think I have proved it, nothing but guaiacum + Ô. Now if you heat the purest oxide of gold, platinum, silver, mercury, the peroxides of manganese, lead, &c., in fact any substance yielding oxygen, within a small glass tube into which you had previously introduced a bit of filtering-paper impregnated with the said guaiacum solution, you will see that bit of paper turn blue as soon as the disengagement of oxygen begins to take place; and all the circumstances being the same, you will further perceive that the paper is coloured most deeply and rapidly by the oxygen eliminated from that oxy-compound which requires the lowest temperature for yielding part or the whole of its oxygen. Thus the oxygen disengaged from the oxides of gold, platinum and silver, acts more energetically upon the guaiacum solution than does the oxygen eliminated from the oxide of mercury, the peroxide of manganese, &c. I trust these results will be obtained in the Royal Institution just as well as I get them in the laboratory of Bâle, or else my discovery will be a very poor thing. As there cannot, I should think, be any doubt that all the oxygen, contained, for instance, in the oxide of silver previously to that compound being decomposed by heat, exists but in one state, be that state what it may, how then does it happen, we may ask, that at the same time two different sorts of oxygen, O ond Ô, are disengaged from the compound named? The answer to this question seems to me to be, that one of the two kinds of oxygen eliminated must be engendered at the expense of the other; or to speak more correctly, that during the act of the elimination of oxygen from the oxide of silver, part of that oxygen suffers a change of condition. Now as the oxides of gold, silver, &c. enjoy the power of colouring blue the guaiacum solution, just as free Ô does, I draw from that fact the conclusion, that the condition of the oxygen contained in the oxides of gold, silver, &c. is the ozonic one; and further infer, that by far the greatest portion of that Ô, under the influence of heat, is transformed into O. Why the whole of the

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oxygen disengaged from those oxides does not happen to be O I certainly cannot tell, but I think that the very fact of the mixed nature of the oxygen in question is, in a theoretical point of view, highly important, and speaks in favour of my notions rather than against them.

Although I have already heavily taxed your patience, I am afraid I cannot yet release you from further listening to my philosophical talkings, for I have still to speak of a subject that has of late deeply excited my scientific curiosity, and taken up all my leisure time. But to give you an idea of what I have been doing these last two months, I must be allowed prefacing a little. You know that I entertain a sort of innate dislike to touch anything in the slightest way connected with organic chemistry, knowing too well the difficulty of the subject and the weakness of my power to grapple with it; but in spite of this well-grounded disinclination, I have of late, and, as it were, by mere chance, been carried into the midst of that field, upon the intricacies and depths of which I have been used all my life to look with feelings of unbounded respect and even awe. The picking up of a mushroon has led to that very strange aberration of mine, and you will ask how such a trifling occurrence could do that. The matter stands thus: what the botanists tell me to be called "*Boletus luridus*," with some other sorts of mushroom, have the remarkable property of turning rapidly blue when their head and stem happen to be broken and exposed to the action of the atmospheric air. On one of my ramblings I found a specimen of the said *Boletus*, perceived the change of colour alluded to, and being struck with the curious phænomenon, took the bold resolution to ascertain, if possible, its proximate cause. I carried home the part, set to work, and found more than I looked for, which luckily enough happens now and then. Being, by the short space allotted even to the longest letter, prevented from entering into the details of the subject, I confine myself to stating the principal results obtained from my mushroom researches. *Boletus luridus* contains a colourless principle, easily soluble in alcohol; and in its relations to oxygen, bearing the closest resemblance to guaiacum, as appears from the fact, that all the oxidizing agents which have the power of bluing the alcoholic solution of guaiacum, also enjoy the property of colouring blue the alcoholic solution of our mushroom principle; and all the deoxidizing substances by which the blue solution of guaiacum is decolorized also discharge the colour of the blued solution of the *Boletus* matter. From this fact and others, I infer that this mushroom principle, like guaiacum, is capable of combining with Ô, and is not affected by O. Now the occurrence of a matter so closely related to guaiacum in a mush-

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room is a fact pretty enough of itself, but as to scientific importance far inferior to what I am going to tell you.

The fact that the resinous *Boletus* principle, after having been removed from the mushroom (by the means of alcohol), is not able to colour itself spontaneously in the atmospheric air, whilst it seems to have that power so long as it happens to be deposited in the parenchyma of the *Boletus*, led me to suspect that there exists in the *Boletus luridus*, besides the guaiacum-like substance, another matter, endowed with the property of exalting the chemical power of common oxygen, and causing that element in its Ô condition to associate itself to the resinous principle of the mushroom. The conjecture was correct; for I found that in the juice obtained by pressure from a number of mushrooms belonging to the genera *Boletus* and *Agaricus*, and notably from *Agaricus sanguineus* (upon which I principally worked), an organic matter is contained which enjoys the remarkable power of transforming O into Ô, and forming with the latter a compound from which Ô may easily be transferred to a number of oxidable matters, both of an inorganic and organic nature; and I must not omit to state, that the peculiar agaricus matter, after having been deprived of its Ô, may be charged with it again by passing through its solution a current of air. The easiest way of ascertaining the presence of Ô in the said agaricus juice, is to mix that liquid with an alcoholic solution of guaiacum, or the resinous matter of the *Boletus luridus*. If the juice happens to be deprived of Ô, the resiniferous solutions will not be coloured blue; but if it contains Ô, the solutions will assume a blue colour, just as if they were treated with peroxide of lead, permanganic acid, hyponitric acid, &c. From the facts stated, it appears that the organic matter in question is a true carrier of active oxygen, and therefore, when charged with it, an oxidizing agent. Indeed, that matter may in many respects be compared to NO^2, which, as is well known, enjoys to an extraordinary extent the power of instantaneously transforming O into Ô, and forming a compound ($NO^2+2Ô$) with that Ô, from which the latter may easily be transferred to a multitude of oxidable matters. Now in a physiological point of view, the existence of such an organic substance is certainly an important fact, and seems to confirm an old opinion of mine, according to which the oxidizing effects of the atmospheric oxygen (of itself inactive) produced upon organic bodies, such as blood, &c., are brought about by means of substances having the power both of exciting and carrying oxygen.

Before dropping this subject, I must not omit to mention a

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fact or two more. The peculiar matter contained in the juice of the *Agaricus sanguineus*, &c., and charged with Ô, gives up that oxygen to guaiacum, and the latter transfers it to the resinous matter of the *Boletus luridus*; thus the different organic matters capable of uniting with Ô as such, exhibit different affinities for that oxygen, a fact not without physiological importance. Another fact worthy of remark, is the facility with which the nature of our agaricus matter may be changed. On heating the aqueous solution which has the power of deeply bluing the guaiacum solution to the boiling-point, it not only loses the property, but also the capacity of again becoming an oxidizing agent, *i. e.* carrier of oxygen, however long it may be kept in contact with atmospheric air. I am very sorry to be prevented from entering more fully into the details of the subject, but from the little I have said about it you may easily understand why this mushroom affair has of late so much engaged my attention.....

Yours most faithfully,

Bâle, Nov. 30, 1855. C. F. SCHÖNBEIN.

XVII. *Analysis of the Meteorites of Mezö-madaras in Transylvania.* *By* Professor WÖHLER *and* Dr. ATKINSON*.

WE have analysed the meteorites which fell at Mezö-madaras in Transylvania on the 24th of September, 1852. Their external appearance afforded sufficient presumption that they were a mixture of several minerals, and this has been confirmed by analysis.

Metallic iron, containing 7·4 per cent. of nickel and 0·25 per cent. of cobalt, forms a chief constituent. The quantity of iron varies in different parts of the specimen, but averages 19·60 per cent. of the entire weight. It was not possible to extract it completely from the powdered meteorite by means of the magnet. We calculated its quantity from the volume of hydrogen evolved when a weighed portion of the meteorite was treated with dilute sulphuric acid. Like all meteoric iron, this also contains phosphorus, the amount of which could not, however, be determined without employing much larger quantities of material. It is not passive, but precipitates copper.

Iron pyrites is a second constituent. It is here and there perceptible to the naked eye; its presence was also shown by the sulphuretted hydrogen, on treating the meteorite with hydrochloric acid. We did not consider it essential to determine its quantity, since it evidently occurs very unequally mixed.

Graphite, to the amount of 0·25 per cent., is a third constituent.

* Communicated by Dr. Atkinson.

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It was seen in brilliant shining lamellæ on boiling out the meteorites with hydrochloric acid.

The chief mass consists of two kinds of silicates, of which one is decomposed and gelatinized by hydrochloric acid, and the other is not decomposed.

A microscopic examination showed that most of the minerals which occur in rounded particles on the dark mass of the meteorite are silicates undecomposable by acids, while the mass of the stone is chiefly made up of decomposable constituents. Exclusive of the determination of the quantity of hydrogen evolved on treating the meteorite with acid, three kinds of analysis were made. One by fusing the meteorite with carbonate of soda, from which the quantity of silica was found to be 41·62 per cent. A second was made with hydrofluoric acid, in which the quantity of silica estimated from the difference was 43·94 per cent. This excess of 2·32 of silica is explained partly from the unequal mixedness of the meteorite, partly from the unavoidable loss in so many constituents, which of course fell on the difference, and partly from the phosphorus, sulphur, and oxide of chromium, the quantities of which were too small to be determined with any degree of accuracy. In this manner, by the usual methods, the meteorites were found to contain in 100 parts the following constituents:—

Metallic iron	18·10
Nickel	1·45
Cobalt	0·05
Graphite	0·25
Magnesia	23·83
Protoxide of iron	4·61
Protoxide of manganese	0·28
Alumina	3·15
Lime	1·80
Soda	2·34
Potassa	0·50
Sulphur, Phosphorus, Oxide of chromium, Silica	43·64
	100·00

An experiment was made to separate the two kinds of silicates. The finely-powdered meteorite was heated for a long time with strong hydrochloric acid. The residue was well washed out, and boiled repeatedly with carbonate of soda. The insoluble residue amounted to 30·48 per cent. (In a second experiment, where the mass was not boiled so long with carbonate of soda, 36 per cent. was obtained.)

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These 30·48 parts of undecomposed residue gave, when analysed with hydrofluoric acid,—

		In 100 parts.
Magnesia	4·600	15·29
Protoxide of iron	4·643	15·25
Lime	0·929	3·05
Alumina	0·564	1·85
Soda	0·585	1·91
Potassa	0·347	1·13
Graphite	0·250	0·82
Cr^2O^3	...	...
SiO^3	18·502	60·70
	30·48	100·00

After subtracting the 19·6 per cent. iron, there remains 50·92 per cent. of silicates soluble in hydrochloric acid, and consisting of—

		In 100 parts.
Magnesia	19·170	37·64
Alumina	2·586	5·08
Lime	0·870	1·70
Soda	1·755	3·44
Potassa	0·153	0·30
Silica	26·386	51·84
	50·920	100·00

From these results, it does not appear to us that any conclusion can be drawn as to the composition of the meteorites. The more so, when we consider that even the insoluble part may contain compounds which are partially decomposed by the continued action of the acid, and afterwards by the alkali. If the quantities of oxygen in the magnesia and iron, the two predominating bases in the insoluble part, be compared with that of the silica, it is almost in the proportion of 1 : 3. We might suppose from this that the chief constituent of the insoluble part is a mineral of the formula $\left.\begin{matrix}MgO\\FeO\end{matrix}\right\}SiO^3$;

and that the soluble part, which is so rich in magnesia, has for its chief constituent a mineral of a formula similar to olivine, $3MgO, SiO^2$.

But it is most probable that the mineral constituents are themselves mixtures, as Rammelsberg assumes to be the case with many similar meteorites. The chief mass of the meteorites of Mezö-madaras would then be composed of a mixture of olivine, augite, and labradorite, containing besides, nickeliferous iron, iron pyrites, graphite, and a small quantity of chrome-iron ore.

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XVIII. *On the Density of certain Substances (Quartz, Corundum, Metals, &c.) after fusion and rapid cooling.* By M. Ch. Sainte-Claire Deville*.

In the *Comptes Rendus*, vol. xx. p. 1453, I communicated the results of some experiments which establish a notable difference between the density of certain crystallized minerals and that of the vitreous bodies obtained by subjecting those minerals to fusion and rapid cooling. I have thus shown that these differences, *referred to the primitive density of the crystallized mineral*, were,—

For a labrador	0·06
For a felspar	0·08
For a hornblende . . .	0·12
For an augite	0·14
For a vesuvian	0·16

From which it may be inferred reciprocally, that, in the act of crystallization, a very remarkable concentration of matter to a maximum of density takes place in these substances.

All these minerals are silicates: it was therefore natural to inquire whether the same effect would show itself with crystallized silica or quartz.

That this is the case I have been able to assure myself, through the obliging assistance of M. Gaudin, who has been kind enough to place at my disposal a simple and ingenious apparatus by means of which he obtains a very high temperature. I have been thus able to obtain with the greatest facility hyaline quartz melted into small drops, or in lumps.

I have carefully determined in the first instance the density of the quartz itself†; here are the numbers which I obtained:—

1. Fine crystal of quartz, perfectly colourless and transparent	2·663
2. Quartz extracted from a granite of a medium grain	2·642
3. Quartz from porphyry, composed of quartz and felspar only	2·668
4. Quartz distributed in an irregular manner in a rock of Guadaloupe, with labrador, and apparently formed by concretion (mean of four experiments)	2·653
Mean	2·656

* *Comptes Rendus*, vol. xl. p. 769.

† The densities cited in this note are taken for the most part with powder of a uniform grain obtained by means of two sieves, rejecting what passed through the finest and what remained upon the coarsest. All the numbers are referred to water at its maximum density.

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Several fragments of No. 1, fused and cooled suddenly, presented the following densities:—

Small rounded globules	2·222
Fragments drawn out and elongated	2·209
Same glass in very small fragments	2·221
Same glass in powder, fine and homogeneous . .	2·228
Mean	2·220

The density of this quartz glass, referred to that of the primitive crystal 2·663, shows a diminution of 0·17.

Of all the minerals which enter abundantly into igneous rocks, quartz seems to be that which possesses in the highest degree the remarkable property of assimilating to itself, during cooling, a certain quantity of heat, which maintains, even after solidification, the molecules at an abnormal distance apart. This property is of a nature to justify the hypothesis of a surfusion, which several geologists, and more particularly M. Fournet, have caused to enter into their appreciation of the circumstances which have accompanied the solidification of the rocks which, like granite, exhibit quartz in considerable proportions.

Sulphur is known to be one of the bodies most easily subjected to the phænomena of surfusion. Experiments which I have already communicated to the Academy (*Comptes Rendus*, vol. xxv. p. 857) gave me, between the density of soft sulphur immediately after its preparation, and that of natural octahedral sulphur, a difference which amounts only to 0·07 of the latter. But this number is evidently a minimum; for, as I indicated in the same note, the transformation of the soft or vitreous sulphur is executed in the first moments with extreme rapidity.

The metals and their compounds (excepting the silicates) seem, on the contrary, to have but little tendency to assume this peculiar abnormal condition. The passage to the crystalline state is almost immediate, however quick the cooling may be.

Crystallized bismuth and the metal suddenly cooled, gave respectively the numbers 9·935 and 9·677. Tin cooled very slowly, and the same metal cooled by being poured into water gave 7·373 and 7·239; which indicate for these two metals, in the two circumstances, a difference of density amounting only to about 0·02 of the maximum.

With lead the phænomenon is still less pronounced; for between lead poured into water and small imperfect crystals of the metal, extracted from masses of the same lead cooled with great slowness, I found a difference of about one hundredth, *but in the inverse sense* (11·363 and 11·254)*.

* Another experiment was made with lead precipitated electro-chemically, and with the same lead melted and run; I obtained the numbers 11·542 and

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Sea-salt in very beautiful colourless crystals gave* . 2·195
The same, fused and rapidly cooled, was evidently in a state of perfect crystallization, and gave . . . } 2·204

that is to say, exactly the same number.

Thus, then, there are substances which, contrary to sulphur, quartz, and the silicates, have only a very feeble tendency, or none at all, to assume, even momentarily, the vitreous state.

It might be demanded, to which of the two categories does alumina belong. Natural corundum in small colourless crystals gave me a density of 4·022; the same crystals, fused with the gas blowpipe of M. Gaudin, had a density of 3·992,—an insensible difference. There is not, therefore, a glass of corundum, as there is of quartz, and this physical property of alumina, as all its chemical properties, directly attaches aluminium to the group of metallic bodies.

[We observe with pleasure the present tendency of experimenters to inquire into the influence of molecular arrangement upon density. The subject is one of great importance, and strongly solicits searching examination. Our handbooks of natural philosophy and chemistry probably contain numerous erroneous statements as to the influence of mechanical pressure upon density; and he who places this question on safe experimental foundations will do a good service to science. We could have wished that the interesting paper before us were a little more precise in the description of the mode of cooling adopted, and of the appearance of the bodies after having been cooled.—Eds.]

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 79.]

May 24, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"On the Theory of the Electric Telegraph." By Professor William Thomson, F.R.S.

The following investigation was commenced in consequence of a letter received by the author from Prof. Stokes, dated Oct. 16, 1854.

11·225, which give a difference equal to 0·027 of the first density, and in the same sense as that of tin and bismuth. But such is the rapidity with which this finely-divided lead is transformed into carbonate in the air, that it was necessary to convert it into sulphate to deduce the weight of the matter employed. Does this complication cast some uncertainty on the first number, or ought we not rather admit it as the density of lead perfectly crystallized?

* In the essence of turpentine, the density of which had been previously determined.

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It is now communicated to the Royal Society, although only in an incomplete form, as it may serve to indicate some important practical applications of the theory, especially in estimating the dimensions of telegraph wires and cables required for long distances; and the author reserves a more complete development and illustration of the mathematical parts of the investigation for a paper on the conduction of Electricity and Heat through solids, which he intends to lay before the Royal Society on another occasion.

Extract from a letter to Prof. Stokes, *dated Largs, Oct.* 28, 1854.

"Let c be the electro-statical capacity per unit of length of the wire; that is, let c be such that c/v is the quantity of electricity required to charge a length l of the wire up to potential v. In a note communicated as an addition to a paper in the last June Number of the Philosophical Magazine, and I believe at present in the Editors' hands for publication, I proved that the value of c is $\frac{I}{2 \log \frac{R'}{R}}$, if I denote the specific inductive capacity of the gutta percha, and R, R' the radii of its inner and outer cylindrical surfaces.

"Let k denote the galvanic resistance of the wire in absolute electro-statical measure (see a paper 'On the application of the Principle of Mechanical Effect to the Measurement of Electromotive Forces and Galvanic Resistances,' Phil. Mag. Dec. 1851).

"Let γ denote the strength at the time t, of the current (also in electro-statical measure) at a point P of the wire at a distance x from one end which may be called O. Let v denote the potential at the same point P, at the time t.

"The potential at the outside of the gutta percha may be taken as at each instant rigorously zero (the resistance of the water, if the wire be extended as in a submarine telegraph, being certainly incapable of preventing the inductive action from being completed instantaneously round each point of the wire. If the wire be closely coiled, the resistance of the water may possibly produce sensible effects).

"Hence, at the time t, the quantity of electricity on a length dx of the wire at P will be $vcdx$.

"The quantity that leaves it in the time dt will be

$$dt \frac{d\gamma}{dx} dx.$$

"Hence we must have

$$-cdx \frac{dv}{dt} dt = dt \frac{d\gamma}{dx} dx \quad \ldots\ldots\ldots\ldots \quad (1).$$

"But the electromotive force, in electro-static units, at the point P, is

$$-\frac{dv}{dx},$$

and therefore at each instant

$$k\gamma = -\frac{dv}{dx} \quad \ldots\ldots\ldots\ldots\ldots \quad (2).$$

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"Eliminating γ from (1) by means of this, we have

$$ck\frac{dv}{dt}=\frac{d^2v}{dx^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (3),$$

which is the equation of electrical excitation in a submarine telegraph-wire, perfectly insulated by its gutta percha covering.

"This equation agrees with the well-known equation of the linear motion of heat in a solid conductor; and various forms of solution which Fourier has given are perfectly adapted for answering practical questions regarding the use of the telegraph-wire. Thus first, suppose the wire infinitely long and communicating with the earth at its infinitely distant end: let the end O be suddenly raised to the potential V (by being put in communication with the positive pole of a galvanic battery of which the negative pole is in communication with the ground, the resistance of the battery being small, say not more than a few yards of the wire); let it be kept at that potential for a time T; and lastly, let it be put in communication with the ground (*i. e.* suddenly reduced to, and ever afterwards kept at, the zero of potential). An elementary expression for the solution of the equation in this case is

$$v=\frac{V}{\pi}\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\frac{\sin\left[2nt-zn^{\frac{1}{2}}\right]-\sin\left[(t-T)2n-zn^{\frac{1}{2}}\right]}{n} \quad . \quad . \quad (4),$$

where for brevity

$$z=x\sqrt{kc} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$"

That this expresses truly the solution with the stated conditions is proved by observing,—1st, that the second member of the equation, (4), is convergent for all positive values of z and vanishes when z is infinitely great; 2ndly, that it fulfils the differential equation (3); and 3rdly, that when $z=0$ it vanishes except for values of t between 0 and T, and for these it is equal to V. It is curious to remark, that we may conclude, by considering the physical circumstances of the problem, that the value of the definite integral in the second member of (4) is zero for all negative values of t, and positive values of z.

"This solution may be put under the following form,

$$v=\frac{2V}{\pi}\int_{t-T}^{t}d\theta\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\cos(2n\theta-zn^{\frac{1}{2}}) \quad . \quad . \quad . \quad (6),"$$

which is in fact the primary solution as derived from the elementary type $\cos\left(2\pi\frac{it}{T}-z\sqrt{\frac{\pi i}{T}}\right)e^{-z\sqrt{\frac{\pi i}{T}}}$ given by Fourier in his investigation of periodic variations of terrestrial temperature.

"This, if T be infinitely small, becomes

$$v=\frac{2V}{\pi}T\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\cos(2nt-zn^{\frac{1}{2}}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

which expresses the effect of putting the end O of the wire for

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an infinitely short time in communication with the battery and immediately after with the ground. It may be reduced at once to finite terms by the evaluation of the integral, which stands as follows:—

when t is positive, $\int_0^\infty dn e^{-xn^{\frac{1}{2}}} \cos(2nt - xn^{\frac{1}{2}}) = \frac{\pi^{\frac{1}{2}} x}{4t^{\frac{3}{2}}} e^{-\frac{x^2}{4t}}$,

and when t is negative, $=0$.

And so we have

$$v = \mathrm{T} \frac{\mathrm{V}x}{4\pi^{\frac{1}{2}} t^{\frac{3}{2}}} e^{-\frac{x^2}{4t}} \quad \ldots\ldots\ldots \quad (8),$$

or by (6), when t is not infinitely small,

$$v = \frac{\mathrm{V}x}{2\pi^{\frac{1}{2}}} \int_{t-\mathrm{T}}^{t} \frac{d\theta}{\theta^{\frac{3}{2}}} e^{-\frac{x^2}{4\theta}} \quad \ldots\ldots\ldots \quad (9),$$

or which is the same,

$$v = \frac{\mathrm{V}x}{2\pi^{\frac{1}{2}}} \int_0^{\mathrm{T}} \frac{d\theta}{(t-\theta)^{\frac{3}{2}}} e^{-\frac{x^2}{4(t-\theta)}} \quad \ldots\ldots \quad (10).$$

It is to be remarked that in (9) and (10) the limits of the integral must be taken 0 to t (instead of $t-\mathrm{T}$ to t, or 0 to T), if it be desired to express the potential at any time t between 0 and T, since the quantity multiplied by $d\theta$ in the second number of (6) vanishes for all negative values of θ.

"These last forms may be obtained synthetically from the following solution, also one of Fourier's elementary solutions:—

$$v = \frac{e^{-\frac{x^2}{4t}}}{t^{\frac{1}{2}}} \cdot \frac{\mathrm{Q}}{\pi^{\frac{1}{2}}} \cdot \sqrt{\frac{k}{c}} \quad \ldots\ldots\ldots \quad (11),$$

which expresses the potential in the wire consequent upon instantaneously communicating a quantity Q of electricity to it at O, and leaving this end insulated. For if we suppose the wire to be continued to an infinite distance on each side of O, and its infinitely distant ends to be in communication with the earth, the same equation will express the consequence of instantly communicating 2Q to the wire at O. Now suppose at the same instant a quantity $-2\mathrm{Q}$ to be communicated at the point O′ at a distance $\frac{\alpha}{\sqrt{kc}}$ on the negative side of O, the consequent potential at any time t, at a distance $\frac{x}{\sqrt{kc}}$ along the wire from O, will be

$$v = \frac{\mathrm{Q}}{\pi^{\frac{1}{2}}} \left\{ \frac{e^{-\frac{x^2}{4t}}}{t^{\frac{1}{2}}} - \frac{e^{-\frac{(x+\alpha)^2}{4t}}}{t^{\frac{1}{2}}} \right\} \quad \ldots\ldots\ldots \quad (12);$$

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and if a be infinitely small, this becomes

$$v=\frac{Qa}{2\pi^{\frac{3}{2}}}\cdot\frac{ze^{-\frac{z^2}{4t}}}{t^{\frac{3}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

which with positive values of z, expresses obviously the effect of communicating the point O with the positive pole for an infinitely short time, and then instantly with the ground.

"The strength of the current at any point of the wire, being equal to $-\frac{1}{k}\cdot\frac{dv}{dx}$, as shown above, in equation (2), will vary proportionally to $\frac{dv}{dx}$ or to $\frac{dv}{dz}$. The time of the maximum electrodynamic effect of impulses such as those expressed by (11) or (13) will be found by determining t, in each case, to make $\frac{dv}{dz}$ a maximum. Thus we find

$$t=\frac{z^2}{6}=\frac{kcx^2}{6}*,$$

as the time at which the maximum electrodynamic effect of connecting the battery for an instant at O, and then leaving this point insulated, is experienced at a distance x.

"In these cases there is no regular 'velocity of transmission.' But, on the other hand, if the potential at O be made to vary regularly according to the simple harmonic law ($\sin 2nt$), the phases are propagated regularly at the rate $2\sqrt{\frac{n}{kc}}$, as is shown by the well-known solution

$$v=\epsilon^{-xn^{\frac{1}{2}}}\sin(2nt-xn^{\frac{1}{2}}) \quad . \quad . \quad . \quad . \quad . \quad (14).$$

The effects of pulses at one end, when the other is in connexion with the ground, and the length finite, will be most conveniently investigated by considering a wire of double length, with equal positive and negative agencies applied at its two extremities. The synthetical method founded on the use of the solution (11) appears perfectly adapted for answering all the practical questions that can be proposed.

"To take into account the effect of imperfect insulation (which appears to have been very sensible in Faraday's experiments), we may assume the gutta-percha to be uniform, and the flow of electricity across it to be proportional to the difference of potential at its outer and inner surfaces. The equation of electrical excitation will then become

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv \quad . \quad . \quad . \quad . \quad . \quad . \quad (15),$$

* We may infer that the retardations of signals are proportional to the squares of the distances, and not to the distances simply; and hence different observers, believing they have found a "velocity of electric propagation," may well have obtained widely discrepant results; and the apparent velocity would, *cæteris paribus*, be the less, the greater the length of wire used in the observation.

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and if we assume

$$v = \epsilon^{-\frac{h}{kc}t}\phi \quad \ldots\ldots\ldots \quad (16),$$

we have

$$kc\frac{d\phi}{dt} = \frac{d^2\phi}{dx^2} \quad \ldots\ldots\ldots \quad (17),$$

an equation, to the treatment of which the preceding investigations are applicable."

Extract from Letter to Prof. Stokes, *dated Largs, Oct.* 30, 1854.

"An application of the theory of the transmission of electricity along a submarine telegraph-wire, shows how the question recently raised as to the practicability of sending distinct signals along such a length as the 2000 or 3000 miles of wire that would be required for America, may be answered. The general investigation will show exactly how much the sharpness of the signals will be worn down*, and will show what maximum strength of current through the apparatus, in America, would be produced by a specified battery action on the end in England, with wire of given dimensions, &c.

"The following form of solution of the general equation

$$kc\frac{dv}{dt} = \frac{d^2v}{dx^2} - hv,$$

which is the first given by Fourier, enables us to compare the times until a given strength of current shall be obtained, with different dimensions, &c. of wire;—

$$v = \epsilon^{-\frac{ht}{kc}} \cdot \Sigma A_i \sin\left(\pi\frac{ix}{l}\right) \cdot \epsilon^{-\frac{i^2\pi^2 t}{kcl^2}}.$$

If l denote the length of the wire, and V the potential at the end communicating with the battery, the final distribution of potential in the wire will be expressed by the equation

$$v = V\frac{\epsilon^{(l-x)\sqrt{h}} - \epsilon^{-(l-x)\sqrt{h}}}{\epsilon^{l\sqrt{h}} - \epsilon^{-l\sqrt{h}}},$$

which, when $h=0$, becomes reduced to

$$v = V\left(1 - \frac{x}{l}\right),$$

corresponding to the case of perfect insulation. The final maximum strength of current at the remote end is expressed by

$$\gamma = \frac{V}{kl} \cdot \frac{2l\sqrt{h}}{\epsilon^{l\sqrt{h}} - \epsilon^{-l\sqrt{h}}},$$

or, when $h=0$, $\gamma = \frac{v}{kl}$.

Hence if we determine A_i so that

* See the diagram of curves given at p. 156.

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$$\Sigma A_i \sin\left(\pi\frac{i}{l}x\right) = -V\frac{e^{(l-x)\sqrt{h}} - e^{-(l-x)\sqrt{h}}}{e^{l\sqrt{h}} - e^{-l\sqrt{h}}} \text{ when } x>0 \text{ and } x<l,$$

the equation

$$v = V\frac{e^{(l-x)\sqrt{h}} - e^{-(l-x)\sqrt{h}}}{e^{l\sqrt{h}} - e^{-l\sqrt{h}}} + e^{-\frac{ht}{kc}}\Sigma A_i \sin\left(\pi\frac{ix}{l}\right)e^{-\frac{i^2\pi^2 t}{kcl^2}}$$

will express the actual condition of the wire at any time t after one end is put in connexion with the battery, the other being kept in connexion with the ground.

"We may infer that the time required to reach a stated fraction of the maximum strength of current at the remote end will be proportional to kcl^2. We may be *sure* beforehand that the American telegraph will succeed, with a battery sufficient to give a sensible current at the remote end, when kept long enough in action; but the time required for each deflection will be sixteen times as long as it would be with a wire a quarter of the length, such, for instance, as in the French submarine telegraph to Sardinia and Africa. One very important result is, that by increasing the diameter of the wire and of the gutta-percha covering in proportion to the whole length, the distinctness of utterance will be kept constant; for n varies inversely as the square of the diameter, and c (the electro-statical capacity of the unit of length) is unchanged when the diameters of the wire and the covering are altered in the same proportion.

"Hence when the French submarine telegraph is fairly tested, we may make sure of the same degree of success in an American telegraph by increasing all the dimensions of the wire in the ratio of the greatest distance to which it is to extend, to that for which the French one has been tried." It will be an economical problem, easily solved by the ordinary analytical method of maxima and minima, to determine the dimensions of wire and covering which, with stated prices of copper, gutta percha, and iron, will give a stated rapidity of action with the smallest initial expense.

"The solution derived from the type $\frac{\epsilon^{-\frac{x^2}{4t}}}{t^{\frac{1}{2}}}$ may be applied to give the condition of the wire, when one end, E, is kept connected with the ground, and the other, O, is operated on so that its potential may be kept varying according to a given arbitrary function of the time: only this, which I omitted to mention in my last letter, must be attended to: instead of merely considering sources (so to speak) at O and O′ (the latter in an imaginary continuation of the wire), we must suppose sources at O, O_1, O_2, &c., and at O', O_1', O_2', &c. arranged according to the general principle of successive images, so that the potential at E may be zero, and that at O may be uninfluenced by all other sources except the source at O itself. Taking .... $O_2, O_1, O, O', O_1', O_2'$.... equidistant, we have only to suppose equal sources, each represented by the type

$$\frac{xe^{-\frac{x^2}{4t}}}{t^{\frac{3}{2}}},$$

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to be placed at these points. For the effects of O_1 and O' will balance one another as far as regards the potential at O.

"So will those of O_2 and O'_1.

" " O_3 and O'_2.

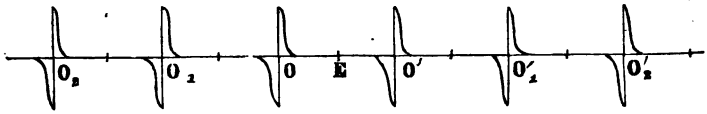
&c. &c.

And again, O and O' would alone keep the potential at E, zero.

So would O_1 and O'_1.

" O_2 and O'_2.

&c. &c.



Hence if we denote $2lkc$ by a, for brevity, the general solution is

$$v=\frac{1}{2\pi^{\frac{1}{2}}}\int_0^t \frac{d\theta F(\theta)}{(t-\theta)^{\frac{3}{2}}}\left\{\ldots(z+2a)e^{-\frac{(z+2a)^2}{4(t-\theta)}}+(z+a)e^{-\frac{(z+a)^2}{4(t-\theta)}}+z\,e^{-\frac{z^2}{4(t-\theta)}}\right.$$
$$\left.+(z-a)e^{-\frac{(z-a)^2}{4(t-\theta)}}+(z-2a)e^{-\frac{(z-2a)^2}{4(t-\theta)}}+\ldots\ldots\right\},$$

where $F(\theta)$ is an arbitrary function such that $F(t)$ expresses the potential sustained at O by the battery.

"The corresponding solution of the equation

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv$$

is

$$v=\frac{1}{2\pi^{\frac{1}{2}}}e^{-\frac{ht}{kc}}\int_0^t \frac{d\theta e^{\frac{h\theta}{kc}}F\theta}{(t-\theta)^{\frac{3}{2}}}\,\Sigma_{-\infty}^{\infty}\left\{(z-ia)e^{-\frac{(z-ia)^2}{4(t-\theta)}}\right\},$$

by which the effect of imperfect insulation may be taken into account."

Extract of Letter from Prof. Stokes to Prof. W. Thomson (dated Nov. 1854).

"In working out for myself various forms of the solution of the equation $\frac{dv}{dt}=\frac{d^2v}{dx^2}$ under the conditions $v=0$ when $t=0$ from $x=0$ to $x=\infty$; $v=f(t)$, when $x=0$ from $t=0$ to $t=\infty$, I found that the solution with a single integral only (and there must necessarilybe this one) was got out most easily thus:—

"Let v be expanded in a definite integral of the form

$$v=\int_0^\infty \varpi(t,\alpha)\sin\alpha x\,d\alpha,$$

which we know is possible.

"Since v does not vanish when $x=0$, $\frac{d^2v}{dx^2}$ is not obtained by differentiating under the integral sign, but the term $\frac{2}{\pi}\alpha v_{x=0}$must be sup-

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plied*, so that (observing that $v_{x=0}=f(t)$ by one of the equations of condition) we have

$$\frac{d^2v}{dx^2}=\int_0^\infty \left\{\frac{2}{\pi}\alpha f(t)-\alpha^2 w.\right\}\sin \alpha x d\alpha.$$

Hence

$$\frac{dv}{dt}-\frac{d^2v}{dx^2}=\int_0^\infty \left\{\frac{dw}{dt}+\alpha^2 w-\frac{2}{\pi}\alpha f(t)\right\}\sin \alpha x d\alpha,$$

and the second member of the equation being the direct development of the first, which is equal to zero, we must have

$$\frac{dw}{dt}+\alpha^2 w-\frac{2}{\pi}\alpha f(t)=0,$$

whence

$$w=e^{-\alpha^2 t}\int^t \frac{2}{\pi}\alpha f(t)e^{\alpha^2 t}\,dt,$$

the inferior limit being an arbitrary function of α. But the other equation of condition gives

$$w=e^{-\alpha^2 t}\int_0^t \frac{2}{\pi}\alpha f(t)\,e^{\alpha^2 t}\,dt=\left(\frac{\pi}{2}\right)^{-1}\alpha\int_0^t e^{-\alpha^2\overline{t-t'}}f(t')dt',$$

therefore

$$v=\left(\frac{\pi}{2}\right)^{-1}\int_0^\infty\int_0^t f(t')\alpha e^{-\alpha^2\overline{t-t'}}\sin \alpha x\,d\alpha\,dt'.$$

But $$\int_0^\infty e^{-a\alpha^2}\cos b\alpha\,d\alpha=\frac{1}{2}\left(\frac{\pi}{a}\right)^{\frac{1}{2}}e^{-\frac{b^2}{4a}},$$

therefore

$$\int_0^\infty e^{-a\alpha^2}\sin b\alpha\,.\,\alpha d\alpha=-\frac{d}{db}\left\{\frac{1}{2}\left(\frac{\pi}{a}\right)^{\frac{1}{2}}e^{-\frac{b^2}{4a}}\right\}$$
$$=\frac{\pi^{\frac{1}{2}}b}{4a^{\frac{3}{2}}}e^{-\frac{b^2}{4a}},$$

whence writing $t-t'$, x, for a, b, and substituting, we have

$$v=\frac{x}{2\pi^{\frac{1}{2}}}\int_0^t (t-t')^{-\frac{3}{2}}e^{\frac{x^2}{4(t-t')}}f(t')\,dt'.$$

"Your conclusion as to the American wire follows from the differential equation itself which you have obtained. For the equation $kc\frac{dv}{dt}=\frac{d^2v}{dx^2}$ shows that two submarine wires will be similar, provided the squares of the lengths x, measured to similarly situated points and therefore of course those of the whole lengths l, vary as the times divided by ck; or the time of any electrical operation is proportional to kcl^2.

"The equation $kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv$ gives $h\propto l^{-2}$ for the additional condition of similarity of leakage."

*According to the method explained in a paper "On the Critical Values of the Sums of Periodic Series," Camb. Phil. Trans. vol. viii. p. 533.

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The accompanying set of curves represents the strength of the current through the instrument at the remote end of a wire as it gradually rises, or gradually rises and falls, after the end operated on is put in connexion with one pole of a battery, and either kept so permanently, or detached and put in connexion with the ground after various short intervals of time.

The abscissas, measured on OX, represent the time reckoned from the first application of the battery, and the ordinates, measured parallel to OY, the strength of the current.

The time corresponding to a is equal to $\frac{kcl^2}{\pi^2}\log_e\left(\frac{4}{3}\right)$, if l be the length of the wire in feet, k its "resistance" per foot, in electrostatical units, and c its electro-statical capacity per foot (which is equal to $\frac{I}{2\log\frac{R'}{R}}$, if I be the electro-statical inductive power of the gutta percha, probably about 2, and R, R′ the radii of its outer and inner surfaces). The principal curve (I.) represents the rise of the current in the remote instrument, when the end operated on is kept permanently in connexion with the battery. It so nearly coincides with the line of abscissas at first as to indicate no sensible current until the interval of time corresponding to a has elapsed; although, strictly speaking, the effect at the remote end is instantaneous (*i.e.* according to data limited as regards knowledge of electricity, to such as those assumed in hydrodynamics when water is treated as if incompressible, or the velocity of sound in it considered infinitely great, which requires instantaneous effects to be propagated through the whole mass of the water, on a disturbance being made in any part of it). After the interval a, the current very rapidly rises, and after about $4a$ more, attains to half its full strength. After $10a$ from the commencement, it has attained so nearly its full strength, that the farther increase would be probably insensible. The full strength is theoretically reached only after an infinite time has passed. The first (1) of the smaller curves represents the rise and fall of the current in the remote instrument when the end operated on is put in connexion with the ground after having been for a time a in connexion with the battery; the second (2) represents similarly the effect of the battery for a time $2a$; the third (3) for a time $3a$ and so on. The curve (II) derived from the primary curve (I) by differentiation (exhibiting in fact the steepness of the primary curve at its different points, as regards the line of abscissas), represents the strength of current at different times through the remote end of the wire, consequent upon putting a very intense battery in communication with the end from which the signal is sent, for a very short time, and then instantly putting this end in communication with the ground. Thus, relatively to one another, the curves (1) and (II) may be considered as representing the relative effects of putting a certain battery in communication for the time a, and a

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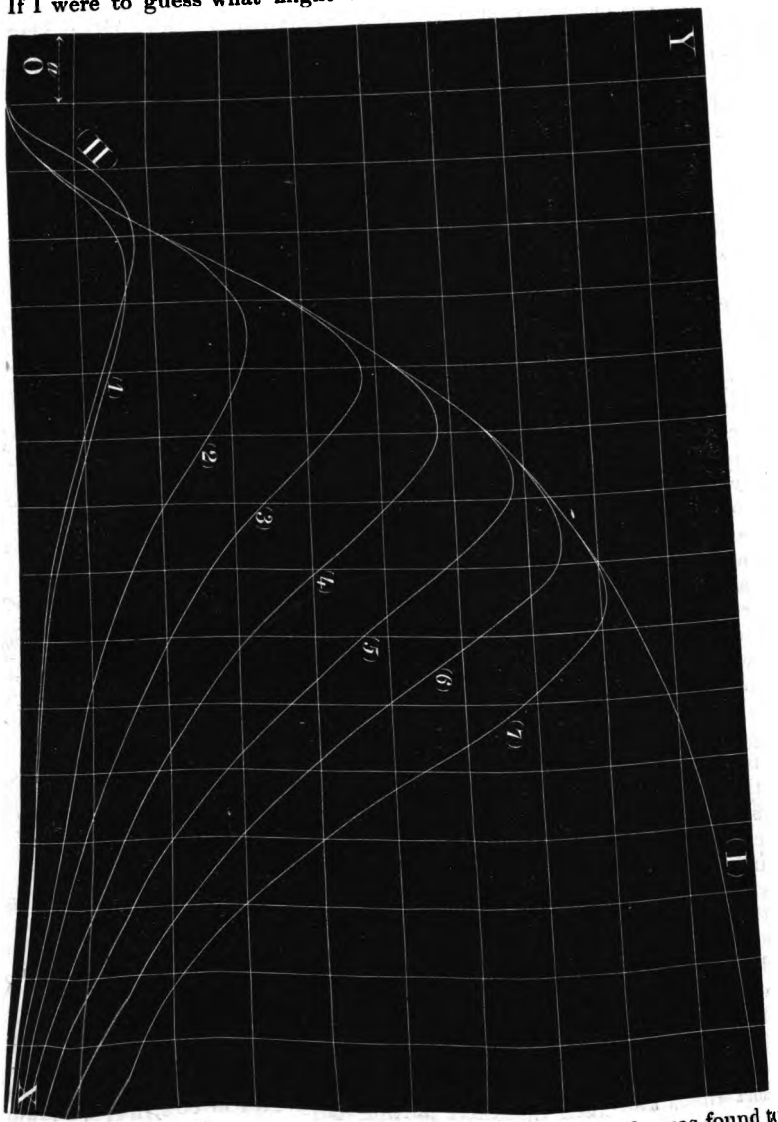
battery of ten or twenty times as many cells for a time $\frac{1}{10}a$ or $\frac{1}{20}a$. If I were to guess what might be called "the retardation," which



in the observations between Greenwich and Brussels was found to be about $\frac{1}{10}$th of a second, I should say it corresponded to four or five times a, but this must depend on the kind of instrument used,

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and the mode of making and breaking contacts with the battery which was followed.

Equation of principal curve (I).

$$y=10a-20a(e-e^4+e^9-e^{16}+\&c.),\text{ where } e=\left(\frac{3}{4}\right)^{\frac{x}{a}};$$

a being half the side of one of the squares.

If $y=f(x)$ denote the equation of the principal wave, and if $f(x)$ be supposed to vanish for all negative values of x, the series of derived curves are represented by the equations

$$\begin{aligned}
&(1)\ \ldots\ y=f(x)-f(x-a)\\
&(2)\ \ldots\ y=f(x)-f(x-2a)\\
&(3)\ \ldots\ y=f(x)-f(x-3a)\\
&\qquad\ldots\ldots\ldots\\
&(7)\ \ldots\ y=f(x)-f(x-7a)\\
&(\mathrm{II})\ \ldots\ y=a\frac{df(x)}{da}.
\end{aligned}$$

I think clearly the right way of making observations on telegraph retardations would be to use either Weber's electro-dynamometer, or any instrument of suitable sensibility constructed on the same principle, that is, adapted to show deflections experienced by a moveable part of a circuit, in virtue of the mutual electro-dynamic force between it and the fixed part of the same circuit due to a current flowing for a very short time through the circuit. Such an instrument, and an ordinary galvanometer, (showing impulsive deflections of a steel needle,) both kept in the circuit at the remote end of the telegraph-wire from that at which the signal is made, would give the values of $\int_0^\infty y^2dx$, and $\int_0^\infty ydx$ (or the area), for any of the curves; and the ratio of the time a of the diagrams to the time during which the battery was held in communication with the wire, might be deduced. The method will lose sensibility if the battery be held too long in communication, but will be quite sufficiently precise if this be not more than ten or twenty times a. I believe there will be no difficulty in applying the method to telegraph-wires of only twenty or thirty miles long, where no retardation would be noticed by ordinary observation. Before, however, planning any observations of this kind with a view to having them executed, I wished to form some estimate of the probable value of a certain element,—the number of electro-statical units in the electromagnetic unit of electrical quantity,—which I hoped to be able to do from the observation of $\frac{1}{10}$th of a second as the apparent retardation of signals between Greenwich and Brussels. I therefore applied to the Astronomer Royal for some data regarding the mode of observation on the indications of the needle, and the dimensions and circumstances of insulation of the wire; and he was so good as to send me immediately all the information that was available for my pur-

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pose. This has enabled me to make the estimate, and so has convinced me that a kind of experiment which I proposed in a paper on Transient Electric Currents in the Philosophical Magazine for June 1853, and which I hope to be able before long to put in practice, will be successful in giving a tolerably accurate comparison of the electrostatical and electro-dynamic units; and, with a further investigation of the specific inductive capacity of gutta percha which will present no difficulty, will enable me to give all the data required for estimating telegraph retardations, without any data from telegraphic operations. This experiment is simply to put two plane-conducting discs in communication with the two poles of a Daniell's battery (or any other battery of which the electromotive force is known in electro-magnetic units), and to *weigh* the attraction between them. I now find that 100 cells of Daniell's so applied would give a force of not less than four grains between two discs each a square foot in area, and placed $\frac{1}{100}$th of a foot apart. As the force varies inversely as the square of the distance between the discs, the weighing will be rather troublesome in consequence of instability, but I think with a good balance it will be quite practicable.

In making this estimate, I suppose the retardation observed between Greenwich and Brussels to be chiefly due to the subterranean part of the wire, and I have taken it as if it were actually observed in 180 miles of coated copper wire. Not having worked out the theoretical problem in the case of a number of insulated wires under the same sheathing, I have considered the cases of a single wire excentrically placed in the iron sheathing, and insulated from it by gutta percha, and a single wire in its own gutta percha tube with the others removed, and itself symmetrically sheathed with tow and iron wire in the usual manner. In the former case the electro-statical capacity of the wire would be $\frac{I}{2\log_e \frac{R^2-f^2}{RR'}}$ approximately*, if R, the inner radius of the conducting sheath, be a considerable multiple of R′ the radius of the copper wire, f denoting the distance between their axes. In the latter case it is $\frac{I}{2\log \frac{R_i}{R'}}$, where R_i is the inner radius of the sheath. These become $\frac{1}{2\cdot45}$ and $\frac{1}{1\cdot35}$, if we take I=2 (as it probably is for gutta percha, nearly enough), and R=·5, $R_i=\frac{1}{8}$, R′=·0325, as the information given me by the Astronomer Royal indicates. Whatever the theory may show for the influence of the other wires, the result as regards retardation must be intermediate

* The rigorous expression, which is very easily found by the method of "electrical images," need not be given here.

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between what it would be if the other wires were removed, and if the one used were separated from them by a sheathing of its own. We may therefore apply the theoretical result by taking c something between $\frac{1}{2\cdot45}$ and $\frac{1}{1\cdot35}$. Hence if "the retardation" agree with the time corresponding to a in the diagrams, k must be intermediate between

$$\frac{\pi^2\times\frac{1}{10}}{\frac{1}{2\cdot45}\times(180\times5280)^2\times\log_e\left(\frac{4}{3}\right)} \text{ and } \frac{\pi^2\times\frac{1}{10}}{\frac{1}{1\cdot35}\times(180\times5280)^2\times\log_e\left(\frac{4}{3}\right)};$$

or again, if "the retardation" correspond to 9 a, k must be intermediate between

$$\frac{\pi^2\times\frac{1}{90}}{\frac{1}{2\cdot45}\times(180\times5280)^2\times\log_e\left(\frac{4}{3}\right)} \text{ and } \frac{\pi^2\times\frac{1}{90}}{\frac{1}{1\cdot35}\times(180\times5280)^2\times\log_e\left(\frac{4}{3}\right)}$$

I think it quite certain that what was observed as the retardation must be in reality intermediate between a and $9a$ of the diagrams. Hence the true value of k for 1 foot of the wire must be between the greatest and least of the preceding estimates, that is, between

$$\frac{1}{108\times10^9} \text{ and } \frac{1}{176\times10^{10}}.$$

But the value of K (the "resistance" in British absolute electro-magnetic measure of 1 foot of the wire) must, according to Weber's observations on copper, be about 99810, or nearly enough 100,000*. Hence σ (the number of electro-statical units in the electro-magnetic unit) being equal to $\sqrt{\frac{K}{k}}$, must be between 104,000,000 and 419,000,000.

According to the observations of Weber, Joule, and others, the quantity of water decomposed by a current of unit strength during the unit of time, that is, by the electro-magnetic unit of electricity, is very exactly $\frac{1}{150}$th of a grain. Hence from 2,000,000 to 8,200,000 electro-statical units are required to decompose a grain of water. A positive and a negative electro-statical unit at a foot distance attract one another with a force of $\frac{1}{32\cdot2}$ of the weight of a grain. Hence if the electricities separated in the decomposition of a grain be concentrated in two points a foot asunder, they will attract with a force of more than 10 tons, and less than 42 tons! Faraday long ago conjectured that less electricity passes in the greatest flash of lightning than in the decomposition of a drop of water, which is now I think rendered very probable.

The expression for the force, in British dynamic units, between

* See a paper "On the Application of the general principle of mechanical effect to the Theory of Electromotive Forces, &c.", published in the Philosophical Magazine, Dec. 1851.

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two plates, each of area S, at a small distance, a, asunder, when connected with the two poles of a battery of which the electromotive force in electro-magnetic units is F, is $\frac{S}{8\pi}\left(\frac{F}{\sigma a}\right)^2$*, or in terms of the weight of a grain $\frac{1}{32{\cdot}2} \cdot \frac{S}{8\pi}\left(\frac{F}{\sigma a}\right)^2$. If F be the electromotive force of 100 cells of Daniell's, which, as I have found from Joule's observations, must be about 250,000,000†, and if a be $\frac{1}{10}$th of a foot, and S a square foot, I conclude from the preceding estimates for σ, that the force of attraction between the plates cannot be less than 4·4 grains, nor more than 72 grains.

It would be easy at any time to make a plan for observing telegraph indications by means of either Weber's electro-dynamometer, or an instrument constructed on the same principle, or by measuring thermal effects of intermittent currents, which could be put in practice by any one somewhat accustomed to make observations, and which would give a tolerably accurate determination of the element of time, even in cases where the observable retardation is considerably less than $\frac{1}{10}$th of a second. A single wire in a submarine cable would, as far as regards the physical deductions to be made from this determination, be to be preferred to one of a number of different wires insulated from one another under the same sheathing. I have little doubt but the Varna and Balaklava wire will be the best yet made for the purpose.

Without knowing exactly what the "retardation" may be in terms of the element of time "a" of the diagrams, we may judge what the retardation, if similarly estimated, would be found to be in other cables of stated dimensions. Thus, if the retardation in 200 miles of submarine wire between Greenwich and Brussels be $\frac{1}{10}$th of a second, the retardation in a cable of equal and similar transverse section, extending half round the world (14,000 miles), would be

$$\left(\frac{14000}{200}\right)^2 \times \frac{1}{10} = 490 \text{ seconds, or } 8\tfrac{1}{6} \text{ minutes:}$$

and in the telegraphic cable (400 miles) between Varna and Balaklava, of which the electro-statical capacity per unit of length may be about one-half greater than in the other, while the conducting power of the wire is probably the same, the retardation may be expected to be

$$\left(\frac{400}{200}\right)^2 \times \frac{3}{2} \times \frac{1}{10} = \frac{3}{5} \text{ of a second.}$$

The rate at which distinct signals could be propagated to the remote end would perhaps be one signal in about a quarter of an hour in the former case, and nearly two signals in a second in the latter.

* As was shown at the conclusion of a paper "On Transient Electric Currents," published in June 1853, in the Philosophical Magazine.

† See the paper referred to above, as published in the Phil. Mag., Dec. 1851.

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June 21, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

Discussion of the observed Deviations of the Compass in several Ships, Wood-built and Iron-built; with General Tables for facilitating the examination of Compass-deviations." By G. B. Airy, Esq., F.R.S., Astronomer Royal.

The author refers, in the first place, to his paper in the Philosophical Transactions for 1839, on the Disturbance of the Compass in Iron Ships, for a theory of the forces produced by the transient induced magnetism of iron. Using the term "polar-magnet-deviation" to express a deviation similar to that which would be produced by a magnetized steel bar partaking of the movements of the ship; and using the term "quadrantal deviation" for a deviation following the law of the sine of double the azimuth, and thus having, if "positive," the signs $+-+-$ in the four successive quadrants of azimuth, or, if "negative," the signs $-+-+$ in the four successive quadrants: then it appears that the deviation produced by the transient induced magnetism of a ship will consist of two parts; of which one will be a "polar-magnet-deviation," such as would be produced by a magnetized steel bar whose axis is parallel to the keel of the ship, and whose absolute intensity is proportional to the terrestrial vertical force at the place; and the other will be a "quadrantal deviation," which, in angular deviation, will be absolutely the same in all magnetic latitudes and with all magnitudes of terrestrial magnetic force, and will usually be "positive." Now combining these forces with the force of the "subpermanent magnetism" of a ship, which in its nature is essentially similar to the magnetism of a steel bar, and would therefore, if isolated, produce "polar-magnet-deviation;" and remarking that the combination of two polar-magnet-forces will produce a third polar-magnet-force, the resulting deviations of which will be "polar-magnet-deviations;" it is evident that the analysis of the observed deviations of the compass in any given instance will consist in resolving them into two systems, one of which follows the laws of "polar-magnet-deviation," and the other is a "quadrantal deviation."

The practical solution of this problem, without the assistance of tables, is troublesome. In order to diminish the difficulty, the author has prepared a Table of polar-magnet-deviations, for the whole circumference as regards azimuths, and for all values of "modulus" or proportion of the disturbing force to the earth's horizontal force, up to 0·8.

To discover the elements of "polar-magnet-deviation," that is, the neutral point and the modulus, in any given case, it is necessary so to combine the observations that the "quadrantal deviation" shall be eliminated. Simple rules are given for this; and the process of investigating the elements, with the assistance of the Tables, is illustrated by exhibiting the work from beginning to end, in an actual instance.

When the elements are found, the Tabular Polar-Magnet-Deviations are to be formed from those elements; and the excess of the

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Observed Deviations over these Tabular Deviations ought to consist simply of Quadrantal Deviation and Errors of Observations.

From the neutral point and the modulus, with the absolute measure of the terrestrial horizontal force, it is easy to form the absolute measures of the apparent permanent forces of the ship, in the directions of "headward" and "starboard" respectively. The starboard force (on the assumption of general symmetry) can arise from nothing but subpermanent magnetism; the headward force will consist of subpermanent magnetism added algebraically to a multiple of the terrestrial vertical force, the multiplier being an unknown constant different for each different ship.

The process is then applied by the author to four wood-built sailing ships, two wood-built steamers, and five iron-built steamers, whose compass-deviations have been observed at twenty-nine stations in all. The results are as follows:—

1. In all cases, the principal part of the deviation follows the law of polar-magnet-deviation.

2. When the polar-magnet-deviation is computed accurately from the Table, and subtracted from the observed deviation, the residual quantity in all cases follows very closely the law of quadrantal deviation, leaving very little to be accounted for by errors of observation.

3. For each ship, the coefficient of quadrantal deviation is sensibly the same in all localities. The small deviations from exact equality cannot be referred to geographical position, and evidently depend on accidental changes in the distribution of the iron, especially of that which is near to the compass.

4. As the test of theory must reside in the comparison of residual quantities, and as it appears that these residual quantities obey with great exactness the law which theory assigns, it follows that the theory, "that the deviation may in all cases be represented by a combination of two deviations, of which one is a polar-magnet-deviation, and the other is a quadrantal deviation whose angular coefficient, for the same ship, is constant under all circumstances," is practically accurate.

5. Consequently in every case the deviation at any locality may be perfectly corrected; by the application of a steel magnet to neutralize the polar-magnet-force; and of a mass of soft iron on one side of the compass, and at the same level, to correct the quadrantal deviation (as the author had previously explained).

6. The mass of soft iron will not require to be changed, under any circumstances. It will depend on the variability or constancy of the subpermanent force to determine whether the steel magnet must be or must not be changed in different localities or after the lapse of time.

7. On forming the expressions for the absolute values of the headward and starboard polar-magnet-forces, it appears that in some ships the subpermanent magnetism is really (to sense) a permanent magnetism, but that in others there is a sensible change. In one instance the change was such that, supposing the deviations to be accurately corrected by magnets and soft iron in England, there would have been at the Cape of Good Hope an error whose maxi-

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mum was nearly $5\frac{1}{2}°$; in all other cases the error would be smaller, and in some practically insensible.

8. The proper way of counteracting these changes evidently is, to readjust the magnets; and for this purpose the magnets should be so mounted as to admit of adjustment at any time. The readjustment can be effected in a very short time when a ship is in port, and can probably also be very easily effected at sea, in favourable weather, when a compass of reference can be carried high up one of the masts.

9. The author strongly condemns any system of navigating a ship by forming a table of compass-deviations from observations at one place, and using that table until observations have been obtained at some other place. It does not in the smallest degree guard against the effect of change in the ship's subpermanent magnetism; and it introduces errors which are purely gratuitous and unnecessary, and which are entirely avoided if the compass is corrected by magnets and soft iron. In elucidation of the amount of errors that may be introduced, if from any cause this system is carried to extremes, he remarks that in the instance of the 'Trident,' sailing from the Thames to Rio Janeiro, the table of compass-deviations formed in the Thames would have been so erroneous when the ship arrived at Rio Janeiro, that on one course the error would have been 6° or 7° in one direction, and on another course it would have been 8° or 9° in the opposite direction: yet during this voyage the ship's subpermanent magnetism had not changed at all; and if the compasses had been corrected in the Thames by magnets and soft iron, there would not have been an error of a single degree in any part of the voyage. In other cases, where there was a real change of subpermanent magnetism, the error would have been fully doubled by carrying on the original table of observed compass-deviations.

The communication is closed by the Table of Polar-Magnet-Deviations. It is a table of double entry, one of the arguments being the azimuth of the ship's head from the neutral point, the other argument being the modulus or proportion of the polar-magnet-force to the terrestrial horizontal force. The azimuth is expressed in points and decimals of a point, and is given for every $0^{p}{\cdot}1$ from $0^{p}{\cdot}0$ to $16^{p}{\cdot}0$; the second half of the circle being a repetition of the first, but with sign changed. The modulus is given for every 0·01 from 0·00 to 0·80. The corresponding deviations are given in degrees and minutes. For each modulus there is also given the mean of all the deviations in the semicircumference, for that modulus; by use of which, in comparison with the mean in any given instance, the modulus in that instance is determined.

GEOLOGICAL SOCIETY.

January 9, 1856,—Sir R. I. Murchison, V.P., in the Chair.

The following communications were read:—

1. "On the Physical Geography of the Tertiary Estuary of the Isle of Wight." By H. C. Sorby, Esq., F.G.S.

In this paper were first described the currents due to the action of

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the tide and stranding surface-waves in an estuary, as determined by the structure of sand-beds, and the relations between them and the physical geography of the limiting shores. The direction and character of the currents being known, the physical geography of the area also might be inferred within certain limits. After this were explained the various structures produced by currents in strata formed under their influence, from which the direction, velocity, character, and depth of the currents can be ascertained. This was followed by an account of the directions and other peculiarities of the currents indicated in the various sandy and other strata of the tertiary formations at numerous localities in the district under consideration. From thence the author obtains data from which many peculiarities in the physical geography of the coast-lines of the tertiary land and sea in the area now occupied by Hampshire and the Isle of Wight can be deduced. The chief of these characters are, that during the tertiary period there was formed a wide estuary of a large river, running from the west towards the east; that the land from which the river came must have been to the north, the west, and south-west, whilst the estuary opened into a tidal sea towards the east; and that at the western part of the Isle of Wight area there existed a considerable shoal. This explains why the section of the tertiary deposits at Alum Bay is so very different to that at Whitecliff; where there was no shoal, but a tidal channel too deep to be affected by the action of the waves of the surface.

2. "On the probable Permian character of the Red Sandstone of the South of Scotland." By E. W. Binney, Esq., F.G.S.; in a letter to Sir C. Lyell, F.G.S.

During a late visit to the South of Scotland, the author came to the conclusion that the red sandstones of Canobie on the Esk, Lockerbie, Corncockle Muir, Dumfries, Thornhill, Sanquhar, and Mauchline, as well as those of the West of Scotland generally, with the exception of the Annan beds, containing tracks of the Labyrinthodon, will have to be classed as Permian instead of Triassic. The Permian beds of the north-west of England, as described lately by the author in the Manchester Memoirs, consist of—1. red and variegated marls (gypsiferous in the north, and calciferous in the south of the district), 300 feet thick; 2. magnesian limestone, 10 feet; 3. conglomerate, 350 feet; 4. lower new red sandstone, 500 feet. The conglomerate of the above list is represented, according to the author, by the breccia of the South of Scotland underlying red sandstones. The conglomerate or breccia consists of a cement, similar throughout the whole region, and of fragments of rocks which vary in their character according to the localities; the imbedded fragments having been in every case derived from the local rocks.

The circumstance of the large tract of the South-west of Scotland hitherto mapped as Trias proving to be Permian will be of great importance to the ironstone and coal districts of the vicinity; since in some instances these latter deposits will probably be followed beneath it.

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XX. *Intelligence and Miscellaneous Articles.*

ON THE CAUSE OF THE PHOSPHORESCENCE OF THE AGARIC OF THE OLIVE. BY M. FABRE.

THE phosphorescence of living plants is a very rare phænomenon; it has been proved principally in some species of the great class of Fungi. The Agaric of the olive (*Agaricus olearius*) has been especially indicated as possessing this property; it has been the subject of the researches of Prof. Delille, and more recently of M. Tulasne; the latter, however, in his important memoir upon this subject, indicates some gaps to be filled up, especially in regard to physico-chemical experiments, and the positive proof of the cause of the luminous phænomenon exhibited by this Agaric. This has been the object of M. Fabre's experiments.

After ascertaining, like the observers who had preceded him, that this phænomenon occurs in the Agaric when living and perfectly healthy, and more especially in the lamellæ which clothe the lower surface of its pileus, he proves, contrary to Delille's assertions, that the phosphorescence is not intermittent, but that it continues during the day as well as during the night. His experiments have also given him the following results:—

1. Exposure to the light of the sun has no sensible influence on the phosphorescence of this Agaric when it is afterwards removed to a dark place.

2. The hygrometric state of the air has no influence upon the phænomenon so long as there is no desiccation of the tissues in which it occurs.

3. Heat, within certain limits, does not modify the phosphorescence, but a reduction of temperature below 46° to 50° F. causes it to cease, without, however, destroying the power of again producing it when the temperature rises again above this limit, at least if the temperature has not been long maintained between 32° and 36° F. A heat above 122° F., on the contrary, completely destroys the property of shining in the dark; and in both cases, in the author's opinion, this is caused by an alteration in the tissues or fluids of the Agaric.

4. The phosphorescence is the same in aërated water as in the open air, but it diminishes by degrees if the Agaric be kept long in the same water, and the presence of dissolved carbonic acid is then evident. In water deprived of air by boiling, on the contrary, the phænomenon ceases in a few moments, but reappears immediately on exposure to the air.

5. The phosphorescence ceases *in vacuo* in hydrogen gas and carbonic acid; it afterwards reappears in the air. A prolonged stay in carbonic acid causes it to cease permanently, as also a very short immersion in chlorine, which evidently produces an alteration in the tissues of the Agaric.

6. Pure oxygen does not increase the brilliancy of the light, which appears to be the same in this gas, in the air, and in aërated water.

7. When phosphorescent, the Agaric produces a quantity of carbonic acid much greater than that which it exhales, under similar circumstances, after its period of phosphorescence has passed. On the other hand, the Agaric in its phosphorescent and non-phosphorescent periods, when kept at a temperature below that necessary for

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the phosphorescence, disengages an equal quantity of carbonic acid. The phosphorescence of this plant is therefore probably connected with the production of a larger quantity of carbonic acid, and may be considered as a phænomenon of combustion.

8. Nevertheless no elevation of temperature has been observed in the phosphorescent parts.—*Comptes Rendus*, Dec. 31, 1855, p. 1248.

ON A PROCESS OF ENGRAVING IN RELIEF ON ZINC.

BY J. DEVINCENZI. REPORT BY M. BECQUEREL.

Zincography, or the art of drawing upon zinc so as to print from it, has already been practised for some years. In England and Germany zinc has long been substituted for stone in lithography, but in France this substitution has not been adopted. M. Devincenzi, wishing to obtain plates in relief on zinc for the purpose of typography, has arrived, after many trials, at the process which will be hereafter described. But in the first place we may observe, that M. L. P. Dumont has since tried a very different process. M. Dumont's process consists in drawing on a plate of zinc with an insoluble chalk of his invention, or with lithographic chalk or ink, liquefying the fatty matter of the drawing by heating it slightly, spreading over the plate a powder composed of resin, burgundy pitch, and bitumen, removing the portion of the powder which does not adhere by means of the bellows, and heating afresh to fix that which covers the drawing. The plate when prepared in this manner is immersed in a bath of sulphate of zinc, and connected with the negative pole of a battery, whilst the liquid is in relation with the positive pole. In this manner a relief is obtained, which serves for the formation of a gutta-percha mould, and from this a plate in relief is produced by electrotype.

M. Devincenzi's process is different from the preceding. The surface of an ordinary zinc plate is grained by means of sifted sand, and the drawing is made upon this with lithographic chalk or ink. It is then passed into a weak decoction of galls, and afterwards into gum-water, in order to hinder the portions of the zinc which are not covered with the drawing from taking the varnish, which will be mentioned hereafter. The plate is washed with water, and the chalk or ink is then removed with oil of turpentine, as in the preparation of lithographic stone. When these operations are completed, the plate is moistened, and a varnish composed of asphaltum, drying oil and turpentine, thinned with oil of lavender, is applied to it with a roller. The varnish only adheres to the portions which were covered with chalk or ink. It is left to dry for twelve or fifteen hours, when a brush soaked in a very weak solution of sulphuric acid is passed over the plate to clean the surface which is not covered with varnish, and the plate is then immersed in a solution of sulphate of copper of 15°, at the same time that a plate of copper of the same dimensions is placed parallel to it at a distance of 5 millims., and connected with it by a copper rod. The portion of the zinc not covered by the varnish is chemically acted upon by the solution of sulphate of copper, and electro-chemically by the action of the voltaic couple, whilst the solution has no action upon the varnish. The zinc plate is taken out every minute to remove the copper deposited,

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and in from four to eight minutes the relief is sufficient for the printing of a great number of copies in the ordinary printing-press.

To prove the applicability of this process, the Commission had a very fine drawing made upon grained zinc and treated by M. Devincenzi; when printed, all the copies exhibited an exact reproduction of the drawing.

A trial of the effect of the chemical action of the sulphate of copper alone, without the intervention of the electro-chemical action, gave unsatisfactory results; the outlines of the drawing were not distinct, and several parts were not given. It appears, therefore, that the voltaic action is necessary. Of some plates M. Devincenzi has printed three thousand copies, the last being as fine as the first; and he considers that zinc, from its presenting more resistance than the alloy of lead and antimony employed in *clichés*, will allow at least as many copies to be printed from it.—*Comptes Rendus*, Dec. 31, 1855, p. 1226.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1855.

Chiswick.—December 1. Hazy: very fine: rain. 2. Cloudy: rain. 3. Clear and frosty. 4. Foggy: drizzly. 5. Clear: overcast. 6. Slight snow: windy at night. 7. Clear: fine. 8. Cloudy and cold. 9. Frosty: cloudy and cold: foggy. 10. Overcast: slight snow. 11. Uniform haze. 12. Overcast: sharp frost. 13. Frosty: fine, with sun: clear and frosty. 14. Overcast: rain. 15. Densely overcast: cloudy. 16. Fine: foggy. 17. Foggy. 18. Cloudy and cold. 19. Clear, cold and dry. 20. Frosty, with dry air. 21. Frosty throughout the day: severe Frost at night. 22. Frosty: overcast. 23. Overcast: fine: showery. 24. Very fine: showery at night. 25. Rain. 26. Boisterous, with rain. 27. Rain: cloudy: fine. 28. Very fine. 29. Fine: very fine: slight rain. 30. Cloudy and fine. 31. Fine throughout.

Mean temperature of the month	34°·99
Mean temperature of Dec. 1854	39 ·35
Mean temperature of Dec. for the last twenty-nine years	39 ·64
Average amount of rain in Dec.	1·492 inches.

Boston.—Dec. 1. Cloudy: rain P.M. 2. Fine. 3. Cloudy. 4. Fine. 5. Cloudy. 6. Fine: snow A.M. 7. Cloudy: snow P.M. 8, 9. Cloudy: snow A.M. and P.M. 10. Fine: snow A.M. and P.M. 11—13. Fine. 14. Cloudy: rain A.M. and P.M. 15. Cloudy. 16—21. Fine. 22. Cloudy: snow A.M. 23. cloudy: snow and rain A.M., and rain P.M. 24. Fine. 25, 26. Cloudy: rain A.M. and P.M. 27—29. Fine. 30, 31. Cloudy.

Sandwick Manse, Orkney.—Dec. 1. Drizzle A.M.: clear P.M. 2. Damp A.M.: damp, vapours P.M. 3. Showers A.M.: drizzly showers P.M. 4. Rain A.M.: showers P.M. 5. Snow-showers A.M.: sleet P.M. 6. Snow-showers A.M. and P.M. 7. Sleet-showers A.M.: clear, frost P.M. 8. Cloudy A.M.: cloudy, frost P.M. 9. Cloudy A.M.: fine, frost P.M. 10. Cloudy A.M.: showers P.M. 11, 12. Snow-showers A.M. and P.M. 13. Cloudy A.M.: showers P.M. 14. Rain A.M.: drizzle P.M. 15. Showers A.M.: showers, thunder and lightning P.M. 16. Hail-showers A.M.: sleet-showers P.M. 17. Fine A.M.: fine, cloudy P.M. 18. Cloudy A.M.: sleet-showers P.M. 19. Cloudy A.M. and P.M. 20. Bright A.M.: Cloudy P.M. 21. Bright A.M.: clear P.M. 22. Clear, frost A.M. and P.M. 23. Snow-showers A.M.: rain, clear P.M. 24. Bright A.M.: rain P.M. 25. Bright A.M.: clear P.M. 26. Drizzle A.M.: rain P.M. 27. Rain, drizzle A.M.: fine, cloudy P.M. 28. Damp A.M.: rain P.M. 29. Bright A.M.: cloudy P.M. 30. Bright A.M.: clear, aurora P.M. 31. Bright A.M.: cloudy P.M..

Mean temperature of Dec. for twenty-eight previous years	41°·03
Mean temperature of the month	39 ·41
Mean temperature of Dec. 1854	39 ·13
Average buantity of rain in Dec. for fifteen previous years	4·21 inches.

The following are the averages for November 1855, with which we have been favoured by our correspondent the Rev. Ch. Clouston of Sandwick Manse, whose usual report miscarried owing to the stormy weather which then prevailed:—

Barometer.		Thermometer.		Rain
A.M.	P.M.	A.M.	P.M.	in inches.
29·970	29·976.	43·53	43·46	1·37.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Dec.	Max.	Min.	8½ a.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	9½ a.m.	8½ p.m.	1 p.m.					
☾ 1.	30·072	29·887	29·70	29·90	29·95	46	33	38	39½	35½	nw.	nw.	calm	·03	.....	·05
2.	29·879	29·811	29·53	29·85	29·87	44	26	36	41	42	n.	nnw.	sw.	·04	·07	·05
3.	29·988	29·983	29·68	29·65	29·65	40	31	31	47	48½	ne.	nnw.	wnw.	·02	.....	·29
4.	29·828	29·690	29·50	29·42	29·39	47	36	39	48½	39½	sw.	w.	nw.	·01	.....	·25
5.	29·557	29·509	29·20	29·33	29·27	44	30	37	33	37	w.	nw.	nw.	.....	.....	·11
6.	29·504	29·373	28·99	29·45	29·54	42	28	32	34	37	nw.	nw.	n.	.....	.....	·38
7.	29·580	29·480	29·18	29·60	29·77	42	27	34	36	36	n.	nw.	wnw.	.....	·03	·50
8.	29·841	29·562	29·36	29·88	30·07	35	19	30	36	35	ne.	nnw.	ne.	.....	·18	·04
● 9.	30·100	29·956	29·74	30·13	30·17	35	30	25	35	36	ne.	nnw.	ssw.	.....	.....	.....
10.	30·079	30·037	29·82	30·07	30·02	35	22	30	41	38	n.	nnw.	nnw.	.....	·21	.....
11.	30·033	29·900	29·70	30·00	29·84	33	23	30	32	31	sw.	w.	nw.	.....	·09	.....
12.	29·885	29·692	29·44	29·74	29·90	33	13	23	31	32	sw.	wnw.	nw.	.....	.....	.....
13.	30·097	30·015	29·76	29·93	29·80	36	15	23·5	34	41½	nw.	n.	w.	.....	.....	·25
14.	30·015	29·900	29·62	29·54	29·72	49	40	36·5	47	47	w.	w.	w.	·07	·03	1·05
15.	30·222	30·038	29·77	29·62	29·70	48	25	44	47	42½	w.	w.	w.	.....	·13	·10
☽ 16.	30·251	30·183	29·92	29·84	29·93	47	29	35	42	40½	w.	w.	wnw.	.....	.....	·17
17.	30·086	30·035	29·80	30·12	30·19	43	35	26·5	37	37½	ne.	nw.	e.	.....	.....	·02
18.	30·214	30·075	29·87	30·18	30·16	40	21	35	39½	39	ne.	e.	sse.	.....	.....	.....
19.	30·264	30·180	30·04	29·96	29·84	32	20	25	38	38	e.	se.	s.	.....	.....	·05
20.	30·093	29·918	29·86	29·73	29·71	32	17	24	38	38	e.	e.	sse.	.....	.....	.....
21.	29·803	29·702	29·63	29·66	29·77	26	11	21·5	37	36	e.	se.	s.	.....	.....	.....
22.	29·895	29·752	29·60	29·77	29·65	29	09	25	34	34	nw.	wnw.	ese.	·10	.....	.....
○ 23.	29·576	29·463	29·24	29·08	29·02	50	37	36·5	34	39½	sw.	s.	sse.	·07	·12	·17
24.	29·663	29·627	29·26	28·97	28·86	49	33	35	40½	43½	sw.	sw.	s.	·30	·18	·16
25.	29·626	29·386	29·30	29·02	29·15	50	35	36	42	42	sw.	s.	s.	·19	.....	·53
26.	29·253	29·187	28·85	29·12	28·09	51	45	45	42	43	sw.	s.	ene.	·26	·15	.....
27.	29·601	29·487	29·18	29·08	29·37	53	37	40	44	42½	sw.	s.	ssw.	·01	·04	·22
28.	29·743	29·616	29·28	29·46	29·19	58	39	40	41	44	s.	s.	sse.	.....	.....	.....
29.	29·963	29·909	29·54	29·43	29·33	51	33	39	42	44½	sw.	ssw.	s.	·01	.....	·18
30.	30·264	30·106	29·71	29·47	29·65	49	27	39	40	39½	w.	ssw.	s.	.....	.....	·07
☾ 31.	30·220	30·035	29·86	29·62	29·67	46	29	39	47	44	s.	s.	sse.	.....	.....	.....
Mean.	29·909	29·790	29·54	29·632	29·620	42·41	27·58	33·2	39·35	39·48				1·11	1·23	4·64

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1856.

XXI. *Remarks on the Estimation of Sulphur in Iron, and on the Solubility of Sulphate of Baryta in Nitric Acid.* By E. CHAMBERS NICHOLSON *and* DAVID S. PRICE, *Ph.D.**

THE method usually employed for estimating the amount of sulphur in a metallic substance, is to act upon it either with nitric acid or aqua regia, and then to precipitate the sulphuric acid with a solution of a salt of baryta. By pursuing this plan for the determination of sulphur in cast iron, the solution obtained by oxidizing 10 grammes of the metal with nitric acid was evaporated to dryness for the purpose of rendering the silicic acid insoluble, the residue then digested with dilute hydrochloric acid until free from iron, and the sulphuric acid in the solution thus obtained precipitated with chloride of barium. The quantity of sulphur found by this method will be seen by the Table, column 1, far to exceed that afterwards arrived at by adopting another process. This process consisted in dissolving the same weight of iron in hydrochloric acid, and passing the gases evolved through a solution of acetate of lead slightly acidified with acetic acid. The precipitate of sulphide of lead thus produced was collected on a filter, washed, dried, and the percentage of sulphur calculated from the weight obtained (see column 2). The residue from the iron, consisting of graphite and silicic acid, was collected on a filter, dried, and then fused with a mixture of nitre and carbonate of soda, in order to ascertain if all the sulphur had been eliminated as hydrosulphuric acid. The fused mass was dissolved in water, the solution acidified with hydrochloric acid, and then evaporated to dryness to separate silicic acid. To the solution obtained by digesting this

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residue with water, and after filtration to separate the silica, the addition of chloride of barium produced in no instance more than the faintest indications of the presence of sulphuric acid.

The per-centages indicated in column 2 may be regarded as correct, as we have verified them by the employment of perfectly pure nitric acid, and by adopting the precautions to which we shall immediately allude.

Gray pig-iron from different works.	Per-centage of sulphur.	
	Column 1. By BaO SO^3.	Column 2. By PbS.
1	0·32	0·10
2	0·28	0·044
3	0·52	0·055
4	0·72	0·081
5	0·45	0·036

The cause of the error by the former process we found to be owing to the presence of sulphuric acid in the acids employed, which we failed to detect by the ordinary method of testing for this impurity, namely by dilution of the acids with a large volume of water before adding a solution of a baryta salt.

By evaporating the acids nearly to dryness in a platinum dish and then adding water, the sulphuric acid or sulphate with which they were contaminated was recognized. The fact of the solubility of sulphate of baryta in mineral acids was not known at the time when we made the above examinations, which was in the autumn of 1853*. It is not our intention now to dwell upon the subject of the amount of sulphur in different varieties of pig-iron, and the manner in which the property of the iron is affected by it. This we reserve for a future communication.

From what has already been stated, it will be seen that errors of two kinds may arise when nitric acid is employed: one where evaporation to dryness is necessary, when, if the acid contain sulphuric acid, an excess of sulphur will be obtained; the other where precipitation is proceeded with in very acid solutions, in which case a loss will result from the solubility of sulphate of baryta in the acid. To prevent this latter source of error, it is necessary either to expel the excess of acid by evaporation, or to neutralize it with an alkali previously to precipitating with a baryta solution. With these precautions sulphur can be correctly estimated by oxidation with nitric acid.

The following Table contains the results of some experiments

* By an abstract of a paper in No. 16 of the Proceedings of the Royal Society, Professor Calvert has, we see, been engaged in experiments on the solubility of the sulphate of baryta.

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which were instituted in order to gain an idea of the extent to which the precipitation of BaO SO^3 is influenced by nitric acid.

Order of experiments.	Cubic centimetres of nitric acid, sp. gr. 1·350, employed.	Cubic centimetres of water.	Sulphate of potash equivalent to ·013 grm. of sulphate of baryta.	Observations.
1	14	14	...	Copious precipitate of nitrate of baryta which appeared after the lapse of a short time.
2	...	28	...	Precipitate of BaO NO^5 less than in Exp. 1.
3	...	56	...	No precipitate after 16 hours.
4	...	84	...	do.
5	...	112	...	do.
6	...	168	...	do.
			Sulphate of potash equivalent to ·027 grm. of sulphate of baryta.	
7	...	14	...	Precipitate of BaO NO^5 same as Exp. 1.
8	...	28	...	do. do. but small.
9	...	56	...	No precipitate after 16 hours.
10	...	84	...	do.
11	...	168	...	do.
			Sulphate of potash equivalent to ·040 grm. of BaO SO^3.	
12	...	14	...	Precipitate of BaO NO^5 same as Exp. 1.
13	...	28	...	A cloud of sulphate of baryta after 10 hours.
14	...	56	...	Faint precipitate do.
15	...	168	...	do. do.
16	...	560	...	No precipitate do. after 16 hours.
17	28	28	...	Nitrate of baryta precipitated after some minutes.
18	...	56	...	No precipitate after 16 hours.
19	...	112	...	do.
			Sulphate of potash equivalent to ·120 grm. of BaO SO^3.	
20	14	28	...	No precipitate at first. After some minutes an appreciable one of BaO SO^3.
21	...	14	...	do. do. and a precipitate of nitrate of baryta.

In the above experiments the sulphate of potash was dissolved in water and the nitric acid added to it. The same volume of a solution of BaCl, containing about twenty times the quantity of

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baryta necessary to precipitate the sulphuric acid present, was added to each.

After recording the above observations, a further addition of BaCl was made to each of the solutions in the first fifteen experiments, and the results noted down after a lapse of fourteen hours.

In Exps. 1 and 2 the quantity of $BaO\,NO^5$ was increased. In Exp. 3 a precipitate of $BaO\,NO^5$ was produced. On the application of heat to these solutions the precipitates dissolved, showing that no $BaO\,SO^3$ had been deposited.

In Exps. 4, 5, and 6, there was a turbidity occasioned by $BaO\,SO^3$.

In Exps. 7, 8, 9, 10, 11, 12, 13, 14, and 15, there was a precipitate of $BaO\,SO^3$.

The addition of a large quantity of BaCl to Exp. 19 caused a copious precipitate of $BaO\,SO^3$.

From these experiments, it will be seen that the amount of $BaO\,SO^3$ precipitated is influenced by the quantity of BaCl present in the solution. We have not ascertained whether the presence of other salts would have produced the same effect.

In the experiments where nitrate of baryta alone is precipitated, a confirmation of Berthollet's law of chemical affinity is afforded.

Sulphate of baryta we find to be also soluble in dilute hydrochloric acid and aqua regia, but not to the same extent as in nitric acid. In the estimation of sulphur in substances where it exists in small quantity, such as in vegetable and animal substances and in metals, and where, owing to the necessity of operating upon a large amount of material, and consequently of taking a proportionate quantity of nitric acid, we have no doubt that errors similar to those which we experienced have frequently occurred.

XXII. *On some Experiments made with a view to determine the comparative Value of Peat and Peat-charcoal for Agricultural purposes.* *By* Edmund W. Davy, *A.B., M.B., M.R.I.A., Lecturer on Chemistry in the Carmichael School of Medicine, &c., Dublin**.

At no former period has the importance of animal excrementitious matter to agriculture been so clearly understood as at present; while the growing attention which is now paid to the sanitary condition of towns, and the methods which have been discovered of deodorizing such matter, afford increased facilities of converting it into the most valuable manure.

Many substances, as chlorine, the chloride of lime and of zinc,

* Communicated by the Author.

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&c., possess considerable deodorizing properties, and may in certain cases be usefully employed for sanitary purposes, but are quite unfit to be used in making manures from animal excreta, because they either decompose some of the most valuable constituents of those matters, or are injurious to vegetation.

The most important substances which have yet been proposed, both for deodorizing and the manufacture of manures from putrescent matters, are peat and peat-charcoal.

The deodorizing property of vegetable charcoal, from whatever source, has long been known; that of uncharred peat was first clearly ascertained by my father, Professor Davy, who at the scientific meetings and lectures of the Royal Dublin Society, and subsequently in a pamphlet, called public attention to it; and his statements have since received the most ample confirmation from various sources. Peat, therefore, in its charred or uncharred state, may be used as a deodorizer for sanitary purposes, and it becomes little more than a question of expense which should be employed for this object.

A difference of opinion, however, is entertained whether peat or peat-charcoal is the best adapted to deodorize animal excreta, &c., where the object is to manufacture manures. The advocates for the use of peat-charcoal allege, as one of the most important of its properties, that, when mixed with decomposing animal excreta, it absorbs and retains the ammonia which is evolved from such matter. If peat-charcoal really does this, it effects a valuable object, as the importance of ammonia as a food of plants and a fertilizer of the soil is well established.

With a view to throw some light on this subject, if possible, I made some comparative experiments with peat and peat-charcoal on stale urine, which by decomposition had become highly ammoniacal. This urine was put into a well-stoppered bottle and kept for the experiments. As peat from different localities differ in certain respects, I employed the same sods, charring one part of each, and leaving the other part uncharred. The peat on being converted into charcoal in a close crucible, was, on cooling, immediately put into a dry bottle and kept well corked. The uncharred peat was broken into pieces and placed in a similar bottle, and both on being used were reduced to the state of coarse powder, the particles of each being about the same size. Having taken equal weights of the powdered peat and peat-charcoal, I put them into two similar evaporating dishes, and intimately mixing each with the same quantity of the ammoniacal urine, left the mixtures exposed to the air for some days under an open shed where they were protected from the rain. The proportions I employed were 500 grains of peat or peat-charcoal to 6 drachms by measure (or about 355 grains by

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weight) of urine. I may observe, on mixing the urine with the charcoal a very strong odour of ammonia was immediately disengaged, and the continued evolution of ammonia from the mixture for several days was readily detected by moistened turmeric paper; whereas in the case of the peat, no odour of ammonia was perceptible on making the mixture, nor could the disengagement of the slightest portion of it from the mixture be detected by means of turmeric paper when examined from day to day.

Having previously determined by experiment how much ammonia was contained in a given quantity of the ammoniacal urine; after the mixtures had been exposed to the air for four days, I divided each into two equal parts, and ascertained how much ammonia was present in one part of each, containing three fluid drachms of the urine.

The following are the quantities of ammonia furnished by the same amount of the urine alone, and when mixed with peat and peat-charcoal, and treated in the manner described.

Amount of Ammonia in 3 *drachms by measure.*

In the urine alone		0·947	part of a grain.
do.	with peat-charcoal	0·238	...
	Loss therefore .	0·714	...
do.	with peat . . .	1·105	...

These results show, that when the urine was mixed with peat-charcoal and exposed to the air for only four days, it lost 0·714 part of a grain of ammonia, which is more than three-fourths of the entire quantity contained in the urine; whereas in the case of the peat, instead of there being any loss of ammonia, there was a slight excess over that existing in the urine alone, which is easily explained by the fact that peat itself always contains a minute quantity of ammonia.

In these experiments, the quantities of ammonia were ascertained by boiling the urine and the mixtures for some time in a retort with a strong solution of caustic potash, and collecting the evolved ammonia in a given quantity of diluted sulphuric acid of known strength, and determining its amount by Peligot's method, which is one much used by chemists on account of its accuracy and expedition.

I made also the following experiments, which confirm the results of those just noticed. Having weighed 300 grains of peat and of peat-charcoal, I carefully mixed each with half an ounce by measure of the same urine as that employed in the former experiments, and putting each mixture on a small saucer placed it in a large plate holding some mercury, and having arranged a small tripod supporting an evaporating dish contain-

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ing some diluted sulphuric acid of known strength over each mixture, finally covered the whole with a bell-glass; the mercury serving to exclude the air. Having left the mixtures thus covered for five days, I removed the bell-glasses and examined, by Peligot's method, the acid contained in each evaporating dish. I could not detect any ammonia in that placed over the peat, showing that none had been evolved, and that the peat had completely retained and fixed, as it were, the volatile carbonate of ammonia existing in the urine. On the other hand, in the case of the peat-charcoal, the acid indicated the absorption of 0·288 part of a grain of ammonia, or considerably more than one-fifth of the entire quantity existing in the urine of the mixture which had been evolved.

I repeated this last experiment, mixing 500 grains of peat and of peat-charcoal with 1 fluid ounce of the same ammoniacal urine, and employing a similar arrangement as in the last, with the exception of using diluted muriatic instead of sulphuric acid for absorbing the evolved ammonia. After the mixtures had been left for sixteen days, I removed the bell-glasses, and found that the mixture with peat-charcoal had a slight urinous smell, and was still evolving ammonia, which was apparent both by its odour and its action on turmeric paper suspended over the mixture; whereas the mixture with peat had no smell whatever, and no evolution of ammonia could be detected by means of turmeric paper. On evaporating to dryness in a water-bath the two acids placed over each mixture, I obtained in the case of the peat-charcoal a residue of 5·7 grains of muriate of ammonia, which is equivalent to 1·812 grain of ammonia, or just about three-fourths of the entire ammonia contained in the urine employed which had been evolved and afterwards absorbed by the acid. On the other hand, in the case of the peat there was an inappreciable residue, which on being dissolved in a little water and treated with caustic lime, gave a slight indication of ammonia, showing that only a very minute quantity had been evolved; and this may in part be accounted for by the peat being mixed with a larger proportion of urine in this than in either of the two former experiments.

The loss of ammonia in the case of the peat-charcoal in these two latter comparative experiments is not so great in proportion, considering the time occupied, as in that of the former; but this is easily explained by the surface exposed not being so large, and the experiments being made under bell-glasses, the same facilities for the evolution of ammonia were not present as when the mixture was exposed to the open air; but had the experiments been carried on longer, a much greater loss of ammonia would have taken place; for on opening the bell-glasses in each,

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it was found that the mixture with peat-charcoal was still evolving ammonia.

These experiments show that peat-charcoal (contrary to the many statements which have been made by its advocates) has very little power of absorbing and retaining the ammonia of excrementitious matter when mixed with it; whereas peat possesses this valuable property in an eminent degree, and absorbs and retains it in a most striking manner, which would appear to be owing (at least in part) to peat containing some substance which acts the part of an acid in neutralizing and fixing the ammonia of the volatile carbonate; for I found that when peat in certain proportions was mixed with urine which was highly alkaline (from the quantity of carbonate of ammonia it contained), and the mixture filtered after a short time, that the filtrate, though it contained ammonia, was quite neutral to test-papers, showing evidently that the ammonia of the carbonate had combined with some other acid to form a neutral salt. The evolution of ammonia in the case of peat-charcoal seems to arise from two causes, namely, its inability to retain the volatile carbonate of ammonia existing in decomposing animal matter, and the property I have observed it to possess of decomposing to a certain extent the fixed salts of ammonia, as, for example, the sulphate, phosphate, muriate, and urate which may be present in such matter, and converting them also into the volatile carbonate which is readily evolved. This latter property would seem to depend on the alkaline and earthy carbonates formed during the process of charring; for when the charcoal was boiled for some time in diluted muriatic acid, and well washed with distilled water so as to remove as much as possible those salts, and again dried at a red heat, the power it possessed of decomposing the fixed salts of ammonia, though not completely removed, was, however, greatly diminished, which clearly shows its connexion with those substances. Peat, on the other hand, does not possess this property in the slightest degree. These facts prove the great superiority of peat over peat-charcoal for agricultural purposes as regards the important substance ammonia; for by the use of peat, the ammonia is retained more or less completely in the manure to exercise its fertilizing action on vegetation, whereas the peat-charcoal suffers it to be in greater part dissipated and lost.

The foregoing results and statements, as regards peat-charcoal, are contrary to what might have been anticipated from the experiments of De Saussure and other chemists, who have shown that charcoal possesses the power of absorbing different gaseous substances, and particularly ammoniacal gas, in large proportion; but the circumstances under which they conducted their

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experiments were very different from those in the experiments described in this communication.

De Saussure, who appears to have made the most extended researches on this subject, when he ascertained that charcoal absorbed about ninety times its volume of ammoniacal gas, employed perfectly dry and very dense charcoal made from boxwood (the denser the charcoal the greater its absorbent power), and in order that it might be as free as possible from air, heated the charcoal red-hot, and while in this state plunged it under mercury and thus cooled it out of the contact of the air, and afterwards let it up into the gas. Such perfectly dry charcoal, and so free from air, could never occur in practice, and are not the conditions in which charcoal is placed when used as a deodorizer of animal excreta, &c.; for in addition to its having absorbed much air and moisture from the atmosphere in spite of the most careful mode of keeping, it becomes more or less completely wet on mixing it with excrementitious matters; and the experiments of De Saussure show that the absorbing power of charcoal for different gases is greatly impaired by the presence of moisture. It appeared, however, interesting to me to ascertain what was the relative absorbent power of peat-charcoal, thoroughly dried peat, and of that in its ordinary state of dryness for ammoniacal gas. For this purpose I selected a good and tolerably dense sod of peat or turf, and having converted a part of it into charcoal, I made three small cubes of the same size as nearly as possible, one out of the charcoal, and two out of the uncharred part, one of which I then thoroughly dried by exposing it for many hours to a temperature of 212° F. The cube of charcoal, that it might be as nearly as possible under the same conditions in respect to dryness and absorption of air as the cube of dried peat, I left exposed to the air for some time and afterwards dried it at 212° F. The third cube was left in its ordinary state of dryness, which was found by drying another portion of the sod to contain about 20 per cent. of water. These cubes were then let up into graduated receivers filled with ammoniacal gas standing over mercury, and the following are the results of their absorption, the volume of charcoal or peat being taken as unity.

Absorption of Ammoniacal Gas.

	Volumes.
Peat-charcoal	18·4
Peat dried at 212° F.	33·2
Peat in its ordinary state of dryness containing about 20 per cent. of water	50·0

As the weight of the cube of peat-charcoal to that of the cube of dried peat in this experiment was in the ratio of 13 to 16·6,

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the volume of ammoniacal gas absorbed by equal weights of the peat-charcoal and dried peat ought, by calculation, to be in the ratio of about 23·4 to 33·2.

These results show that the absorbent power of peat-charcoal for ammoniacal gas, even in the dry state, is very much overrated, and is much less than that of dried peat, whether estimated by bulk or weight, and is far less than that of peat in its ordinary state of dryness.

As regards carbonic acid, the great food of plants, peat has a decided advantage over peat-charcoal, as the former readily undergoes decomposition in the soil, particularly if it is in contact with decomposing matter (as excrementitious substances), and gives rise to carbonic acid in the soil, both to supply the wants of the young plant before its leaves are sufficiently formed to obtain this indispensable substance from the surrounding atmosphere, and to render soluble in water certain earthy salts, &c. required by vegetation, and present them in a state in which they can easily be taken up by the roots of plants. Charcoal, on the other hand, from its being so little liable to undergo change, or be oxidized and converted into carbonic acid at the ordinary temperature, would, under the same circumstances, furnish only a very minute quantity of carbonic acid, even after the lapse of a long period.

Peat, likewise, from its greater elasticity, is better calculated than peat-charcoal to improve the texture, and render more pervious to the air heavy clay soils deficient in vegetable matter; and besides many other arguments which might be adduced in its favour, peat in the partially dried* and coarsely-powdered state in which it should be employed, would only be about one-fifth, if so much, of the expense of peat-charcoal. All these circumstances show that peat is greatly superior to peat-charcoal in manufacturing manures for agricultural purposes.

XXIII. *Hydraulic Researches.* *By* G. Magnus.

[Concluded from p. 107.]

Jets from a cruciform aperture.

61. If an aperture be made in the form of a cross, as represented in Plate II. fig. 17, in which the length of each slit, $yy_{,}$ is 40 millims., and the breadth 3 millims., then when the efflux is regular, *i. e.* when neither a rotation of the liquid nor any hindrance to the motion is present, the form will be obtained

* The peat used in all these experiments, except those on the absorption of ammoniacal gas, contained about 28 per cent. of water.

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which is depicted in fig. 17 *a*, as seen by an observer who stands in the prolongation of xy.

From the aperture $xy_I y_{II} y'$ the water falls down in four arms which cross themselves, $y_I e_I$, ye, $y_{II} e_{II}$, &c., of which each has a strong edge. By the encounter of these four edges, every two form a surface, rpq. Since, however, the encounter is central, each surface bisects the angle formed by the edges by which they were made. The four surfaces formed in this manner have, hence, the position, fig. 17, marked by the dotted lines pp. They are at right angles to each other, and bisect the angles of the cross $yy_I y_{II} y'$. The edges ye stretch between them down to g. In the horizontal line going through this point, that is at p, the surfaces epq have their greatest breadth. Below this part they again assume thick edges, by whose encounter new surfaces, zv, are produced. Since the edges pq and $p_I q$, &c. also encounter centrally, the new surfaces zv, &c. bisect again the angle which the edges pq, &c. form with each other, and are hence again at right angles to each other, and have the same situation as the arms ye, $y_I e_I$, $y_{II} e_{II}$ of the cross.

I have seldom succeeded in obtaining more than two such systems of surfaces under each other; perhaps because the efflux orifice was not sufficiently accurately made; for the smallest difference in the dimensions of the individual arms xy or xy_I deranges the symmetry of the surfaces rpq and their edges. The formation of a third and fourth system of surfaces is likewise rendered more difficult, from reasons similar to those to be afterwards mentioned in § 83.

62. If any of the before-mentioned disturbances occur in the efflux, the edges no longer encounter centrally, and the jet assumes a spiral form. There are, however, few cases in which such a spiral form is regular, for it is seldom that the disturbances act symmetrically on all four arms of the cross $yy_I y_{II} y'$. Fig. 17 *c* represents this form as it was obtained by placing pieces of metal plate E exactly of the same size, and in the same position on the four arms ye, $y_I e_I$, $y_{II} e_{II}$, $y' e'$ of the cruciform aperture, fig. 17 *b*. If these pieces were not all exactly alike, or if they were not all placed symmetrically, the jet had not a regular spiral shape. The metal pieces E must not be too thick, for then the individual windings yp, $y_I p_I$, $y' p'$, &c. would separate.

63. How remarkable the form of the jet issuing from the cruciform aperture may be, and yet how similar to the phænomena hitherto observed, is seen when the efflux from one of the four arms of the cross is stopped, so that the efflux orifice has the form y, y_I, y_{II}, fig. 18. The jet assumes, then, the form represented in fig. 18 *a* and fig. 18 *b*. Fig. 18 *a* represents it as seen by an observer standing at right angles to yxy_I, and fig. 18 *b* as seen by one who stands in the prolongation of yxy_I.

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The water which falls from the three arms xy, xy_I, xy_{II} forms, as in a former case, § 61, thick edges, ye, $y_{II}e_{II}$, which encounter centrally. Thereby result the two surfaces rpq and $r_Ip_Iq_I$, which bisect the angle made by the edges ye and $y_{II}e_{II}$, as well as that by the edges $y_{II}e_{II}$ and y_Ie_I, and which have the position xp and xp_I, fig. 18. But by the central encounter of the edges ye and y_Ie_I a third surface, wzv, is formed, which bisects the angle of these edges, or is at right angles to yxy_I, and consequently falls in the prolongation of the plane $xy_{II}e_I$. It extends itself only towards the side wz, and is scarcely perceptible on the other side, where it is met by the edge $y_{II}e_{II}$. The edges of the surfaces rpq and $r_Ip_Iq_I$ form by their encounter a new surface mn, which falls in the same plane with wzv. There are formed, however, by the encounter of the surfaces rpq, $r_Ip_Iq_I$ with the surface wzv, two new surfaces, hik and $h_Ii_Ik_I$. A second system of surfaces is not perceptible in this jet, because by the encounter of surfaces of unequal mass the motion is too irregular, and hence the connexion between the particles of water is too much disturbed.

Jets from a square orifice.

64. When the water issues from a square orifice, and when no hindrance, rotation, or other disturbing motion is present in the vessel, a jet is obtained whose form is similar to that shown in fig. 19, when viewed from a position perpendicularly opposite one of the sides of the square.

Below the place of greatest contraction, ooo, four surfaces, opq, &c., are seen whose productions pass perpendicularly through the centres of the sides of the aperture. Below this system of surfaces is a second and similar one, which also contains four surfaces, uvw, &c. These latter bisect the angles of the first system, and consequently coincide in direction with the diagonals of the square aperture. To understand this better, $\alpha, \beta, \gamma, \delta$ represent the horizontal sections of the jet at those places where the dotted lines $\alpha\alpha_I$, $\beta\beta_I$, $\gamma\gamma_I$, $\delta\delta_I$ cut the jet. Below the second there is a third system of surfaces, which again is similarly situated to the first; and below that a fourth, whose surfaces are parallel with the second, and so on. I have often observed nine such sharply-defined surfaces below each other, and below these a considerable number which were not so sharply defined. After what has been said before, § 61, about the origin of such surfaces from a cruciform aperture, no further explanation is needed to show how the second system of surfaces is produced from the first, and from the second the following ones. But it is not so easy to explain how the first system of surfaces is formed, or how the peculiar form is produced, which the jet shows previous to the commencement of the first system. I will endeavour to explain the origin of this form.

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Explanation.

65. The parts of the liquid arrive at the orifice in very different ways. The motion of each particle, and we will first consider that of a particle at the edge of the aperture, can be divided into a vertical and a horizontal component, and this latter can be again divided into two directions, of which the one is normal to the perimeter of the aperture, and the other at right angles to the normal.

It is of course impossible to determine the magnitude of the motion of such a particle in different directions during the efflux, as also the relation of these motions for the whole of the particles which simultaneously pass over the edge of the aperture, since they may have attained the edge in very different ways. But before the liquid begins to issue, the whole of the particles at the bottom are under the same pressure. All those at the edge of the aperture move, when the latter has been opened, with equal velocity over the edge, since it is assumed that up to then no motion has occurred in the vessel. Hence they all begin their paths in the direction of the normal with equal velocity. But having passed over the edge of the aperture, there is in addition to the horizontal, a vertical motion. The resultants of these two change their direction continually, partly in consequence of the gravity of the particle, and partly in consequence of the pressure which the mass of liquid above exercises. But a change in the direction of the motion is principally effected by the cohesion which takes place between the parts of the liquid, and by the resistance which the mass of water in the interior of the jet opposes. These last actions of cohesion and of resistance induce principally the peculiar forms of the jets, for they are not the same for all particles passing from the edge of the aperture.

66. Let us consider first the resistance of the liquid in the interior of the jet to the horizontal part of the motion of a particle passing from the edge of the orifice to the cross section of the jet immediately under it. It is manifest that if the orifice were a circle, this resistance would be equal for all particles which came from the circumference of the aperture. But if the aperture be not a circle, and those radii of curvature of its perimeter which fall in the aperture be called positive, and those without negative, then the resistance to a force acting from without must be less at those points whose radii of curvature are positive, than at those points at which they are negative; and it will be the less the smaller the positive radius of curvature. For at that part of the perimeter at which the radius of curvature is positive and smaller than at the adjacent parts, which therefore project more, the particles of the liquid are more

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easily pressed to the side, because there are fewer particles near them whose inertia they have to overcome. Hence they oppose less resistance to a force acting at right angles from without than the less projecting parts.

For the same reason, at those parts where the radius of curvature is negative, the resistance to a force acting at right angles from without is greatest when the radius of curvature is least. In a square aperture, the particles falling from the corners of the squares experience a less resistance in a horizontal direction than those descending from the sides. The particles of water coming from the projecting places experience a less resistance in passing from the edge of the aperture to the cross section underneath, and from this to a second, and so on, than the others. On account of this smaller resistance, the motion of the particles coming from the projecting parts of the orifice is more horizontal; and these particles reach the deeper cross section of the jet with less inclination than those from the less projecting parts, and consequently produce a greater pressure against the liquid in the deeper cross section.

67. In the same manner, the cohesion or attracting force which takes place between the particles of the liquid acts so, that if the aperture be not circular, the directions of the particles coming from the edge of this orifice are not all changed alike; for in consequence of this mutual attraction, the particles in any cross section of the jet would only be *in equilibrio* if the cross section were circular. Hence, in the cross sections which are not circular, there results a motion by which the perimeter is speedily changed into the circumference of a circle. But this motion is not the same for all particles in the same perimeter. Those which are at the prominent points, or where the radius of curvature is positive and least, are attracted with greater force to the interior of the jet, than those at the points where the radius of curvature is negative.

Hence the particles coming from the projecting places also experience, in consequence of the cohesion of the liquid, a less hindrance to their horizontal motion than those coming from other points, and assume, in consequence of this mutual attraction, a more horizontal direction than the latter.

68. Having a more horizontal direction, the pressure also which the particles falling from the prominent places of the aperture exercise on the interior of the jet, is greater than that which the rest produce. This greater pressure can even produce a cavity in the jet. This can be best observed by using an aperture of the form fig. 20, wherewith the water coming from the part *bd* produces a considerable cavity in the jet. Fig. 20 *a* represents a vertical section. The particles of water coming

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from *d* move in almost a horizontal direction, *dg*, towards the jet issuing from the circular part of the aperture, and in consequence of the pressure which they exert, the concave surface *gk* is formed.

69. Similar phænomena to those with the aperture just mentioned are seen with all angular orifices, and especially with a square one. The masses of water coming from the corners press stronger against the interior of the jet than those coming from the sides, and hence the former have the same relation to the latter as single jets which move towards each other. By the encounter of these jets is produced the first system of surfaces, and from that all following ones in the same manner as with the cruciform aperture, § 61, fig. 17.

70. That the peculiar action which the parts of water coming from the prominent places of the orifice exert is chiefly dependent on the less resistance which the horizontal part of their motion experiences from the mass of water contained in the jet, is evident from the fact, that when the efflux is so arranged that the parts of the liquid can only move in a vertical direction, this action is no longer exerted. By using an aperture in a thick wall instead of a thin one, the particles have, on leaving the orifice, only a vertical motion. Hence the form of the jet can only be changed by cohesion, and not by an inequality of resistance. But the changes produced by cohesion alone are far less than those produced by resistance and cohesion together.

71. If the water be allowed to issue from a tube 25 millims. long, and which has a square section as large as the aperture in the thin wall, and if care be taken that the tube is completely wetted and full, the jet assumes a circular section just on leaving the tube, and has neither the ventral segments nor any of the phænomena which are presented by the jet issuing from a thin orifice.

The case is similar when, instead of the aperture, fig. 20, a tube is used whose section is equal to this aperture. The jet assumes then a section which is almost circular. Immediately below the aperture it appears broader, which arises from the fact that the particles issuing at *d*, fig. 20, although only acted upon by cohesion, are yet pressed by it towards the interior of the jet with a much greater force than are the other particles. The jet once broader, contracts again, is again broader, and thus these ventral segments are repeated several times.

72. By this, the difference of the efflux from a thin or thick wall is rendered clear. In using the latter, or a tube, and when all the parts move vertically, cohesion alone acts, and produces the changes of form. If, on the other hand, the aperture be made in a thin wall, the particles of the liquid move not only in the vessel in a more or less horizontal direction, but maintain

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this direction in passing through the orifice, and thus the resistance experienced by the particles coming from the prominent corners, produces that greater change of form already described.

73. The peculiar form of the upper part, $xyoozz_{,}$, fig. 19, of the jet from a square aperture is also explained by the influence of pressure, as above elucidated (§§ 65 to 68).

The particles of water coming from the corners of the square exert a greater pressure on the sections immediately beneath the aperture than the others; hence the corners are blunted, as is seen in the section α. In passing from this to the following section, the blunt corners exert a greater pressure than the sides of the square. For this reason the corners become more and more blunt in the lower sections, and the sides of the square become smaller. Hence on the jet are formed the triangular surfaces *xyo*, fig. 19, which are sometimes vertical; but sometimes, when a greater pressure is exerted by the blunt corners on the interior of the jet, appear pressed outwards, so that the point *o* is more prominent than the side of the aperture above it. With a regular efflux, however, the four points *o* lie in the same horizontal plane. The section in this plane is almost square, although its sides are generally bent inwards; the edges *o* correspond, however, to the centres of the sides of the aperture, a phænomenon which has been frequently observed, but never before explained. The somewhat curved surfaces *xoo*, *yoo*, &c. are continued to $zz_{,}$. By the pressure which is exerted in these on the interior of the jet, the masses of water which form the surfaces *opq*, $o_{,}p_{,}q_{,}$ &c. are forced out, and the same action is produced as with a cruciform aperture, § 61, or as if four jets coming from the corners of the square were to move towards each other.

74. If the motion of the several particles of water be considered, it follows directly from the explanation of the origin of the jet just given, § 65 *et seq.*, that scarcely one of them moves in a vertical plane. It is manifest that in that part of the jet where the various surfaces *opq*, $o_{,}p_{,}q_{,}$, &c. are situated, the particles do not remain in the same vertical plane. But even in that part of the jet above the first system of these surfaces, $xyoo_{,}zz_{,}$, they do not remain in the same vertical plane, with the exception of those particles which come from the middle of the sides of the square; for the parts coming from the prominent places of the aperture press aside those particles opposing resistance to them. This lateral motion may be clearly perceived in the surfaces $yoo_{,}z$, for the particles are seen moving from *y*, and from the places situated under *y* in the line *yz* above $oo_{,}$ towards *o* and $o_{,}$. Similarly it is evident, that in the surfaces *xyo* the particles converge towards *o*. These converging and diverging motions of the particles have already been observed by Poncelet and Les-

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bros*, who, however, as observed above, have not stated how these motions of the particles arise. The explanation given of the origin of the jet proves their necessity, so that these motions may be taken as a proof of the correctness of the explanation.

75. The forms of jets from triangular, pentagonal, hexagonal, and all other regular polygonal apertures, may be explained in an exactly similar manner to those from a quadratic aperture. The form of a triangular jet is given in fig. 21. The surfaces of the first system, *opq*, &c., are also in this at right angles to the sides of the aperture *xy*, &c., and the form of the part between the aperture and this first system is intelligible enough after what has been said, § 73 *et seq.*, of a quadratic aperture.

In pentagonal and hexagonal apertures the several surfaces *opq* are still distinctly perceptible. With all these apertures, a second and third system of surfaces, and often more, are visible. The surfaces of each system bisect, as in the triangular and quadratic apertures, the angles between the surfaces of the preceding system; but the more corners the aperture has, the less prominent are the surfaces. If the number of the corners is great, the surfaces of a system lie very near each other, and they then appear like a dilatation of the jet uniformly disposed around it.

Jet from a circular aperture.

76. Circular jets deport themselves differently to those coming from angular apertures. After what has been published by Savart, it might have been supposed that the dilatations just mentioned, which the surfaces belonging to a system in a polygonal jet form, would also occur in circular jets. But this is not the case. For such a jet exhibits no dilatations if it issues from perfectly circular apertures; if all hindrances in the interior of the vessel are avoided, if no rotatory or other motion occurs, there is formed under these circumstances a connected mass of considerable length, without any, or at all events with so inconsiderable dilatations, that they are scarcely perceptible. From an aperture 12 millims. in diameter, and with a pressure of $0^{m}{\cdot}25$, I have obtained a jet which fell vertically $2^{m}{\cdot}5$ without any dilatations. It would doubtless have retained its connectedness to a still greater depth had it been possible to fix the vessel firmly enough at a greater height. The depth to which the jet remains continuous, is changed as well by the pressure as by the diameter of the aperture. The smaller this is, the smaller is the distance to which the jet retains its continuity; and this distance is also smaller the less the pressure under which the efflux takes place.

* *Expériences Hydrauliques*, p. 151.

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The aspect of such a continuous jet, $2^{m}.5$ long, is very beautiful. It appears like a perfectly turned solid mass of the whitest glass, for there is not the slightest motion perceptible.

77. That in a jet issuing from a circular orifice there are no dilatations perceptible in the immediate neighbourhood of the orifice, is evident from the explanation just given of the form of jets issuing from a quadratic or polygonal aperture. For since in a circular jet the resistance in all directions is the same, and all parts of the liquid pass over the edge of the orifice with equal velocity, the resistance for all parts is the same, and no part of the jet can be pushed more forward than another.

78. But if the afflux of the liquid does not take place with equal velocity from all sides of the vessel, either because the edge of the aperture is not quite smooth, or because the aperture is too near one wall of the vessel, or because it has been made in a wall of the vessel, and its diameter is so great that the liquid flows out at the lower edge with a greater velocity than at the upper one, or because other hindrances occur, then dilatations are formed even in a circular jet in the immediate neighbourhood of the aperture. By placing a piece of sheet-metal at the bottom of the vessel near the orifice they may be perceived.

79. These dilatations or ventral segments are, however, not to be mistaken for the ventral segments which Savart has described*. The latter, to which I shall afterwards, § 81, return, are formed only when the liquid has ceased to be continuous; while the dilatations are formed at a very small distance from the aperture, where the jet is still perfectly continuous. These ventral segments differ from those of Savart, in so far that the latter form surfaces of rotation, while the ventral segments under notice have no circular sections.

80. If the water be allowed to issue from a circular aperture tranquilly and without any perceptible disturbance, but without using the tranquilizer, § 38, the rotation, mentioned at § 37, occurs in the vessel after some time, and the jet assumes a small spiral form, which is first perceived at the parts most distant from the efflux orifice. This is of course present in the upper part of the jet, but it is not visible: only after some time, when it is tolerably marked in the lower part, is it perceptible above, and thus it has the appearance of spreading from below upwards. After some time the whole jet appears like a twisted rope, and although it issues from a circular orifice, it is quite similar to the jet from an elongated quadrangular orifice, § 51, fig. 16 *a* and *b*, excepting that it is not deflected like that, but falls down vertically. After what has been said, § 52 to § 58, on the origin of such spirally formed motions, this needs no further explanation. Every

* *Annales de Chimie et de Physique*, 2nd series, vol. liii. p. 337.

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motion which is communicated to the vessel causes a change in the form of the jet. Sometimes the spiral motion in jets issuing from circular orifices is so violent that they separate into two or more jets, which then continue their way separately, just as with the jet from an elongated quadrangular orifice, § 58.

Maximum of contraction.

81. In perfectly regular jets from circular orifices there is no maximum of contraction. Although their diameter decreases most rapidly in the neighbourhood of the orifice, it also continues to decrease until the jet has lost its continuity.

82. Newton* first maintained that the quantity of water flowing out was regulated by the contraction of the jet, and he measured this contraction; but I could not find a decided explanation of the contraction of a jet (*contractio venæ*), either in Newton's work, or in any others on the same subject.

Since it is a question of a determinate plane, we must presuppose, and many have thus understood the expression, that a section of the jet is meant, which is a minimum, *i. e.* smaller than all other sections; so that the jet, after it has contracted to this minimum, assumes either larger sections, or at any rate contracts no more.

Such a smallest section is seen in all jets which present dilatations in their continuous part; hence it is present in all which do not issue from circular orifices; and even in these, when the afflux to the orifice does not occur with equal regularity from all sides. But in circular jets, which issue regularly from a horizontal aperture, there is no maximum of contraction perceptible, but, as before remarked, § 78, their diameter continually decreases till they cease to form a continuous mass. F. Savart† mentions this in his description of jets.

83. It is remarkable that measurements have been made, principally of circular jets, not only by Newton‡, but also by the greater number of those who have measured the contraction of the jet.

Probably they have never used quite regular circular jets, or by contraction they have understood something quite different; for, as already mentioned, § 78, the diameter of perfectly circular jets diminishes most rapidly near the efflux orifice, obviously in consequence of the horizontal motion by which the particles of liquid in the vessel reach the orifice. After this decrease has commenced, the diameter is smaller, because the velocity of the falling liquid is greater. This last decrease in the diameter is

* *Principia Philos. Natur.*, Prop. XXXVI.
† *Annales de Chimie et de Physique*, 2nd series, vol. liii. p. 338.
‡ *Principia Philos. Nat.*, Prop. XXXVI.

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far smaller than the first, and by many it appears to have been quite neglected, for they considered the jet to be cylindrical after experiencing the first contraction; at least Bossut says*, "at the point of contraction the jet assumes a prismatic form and retains it for a short distance." It scarcely needs mention, how difficult it is to determine where the part which we consider as cylindrical begins, and how little we are in a position to measure its diameter with accuracy. We cannot, therefore, in a strict sense of the word, speak of a maximum of contraction in circular jets.

Savart's ventral segments.

84. If, whilst a jet of water is issuing from a circular orifice quite tranquilly and without any dilatations, an agitation lasting only a short time be made, by stamping for instance on the ground, the jet separates close to the orifice and carries an air-bubble down with it. In thin jets this separation is not noticed, but when the diameter of the jet is 12 millims. or more, it is seen very distinctly. It arises evidently from the vibration which the vessel assumes, and hence the liquid in the orifice assumes for a moment a motion which is opposed to that with which the liquid would have passed out of the orifice. If, instead of this agitation, a tone of some duration be produced in the neighbourhood of the vessel, it will also be set in vibrations. They are not so strong as those produced by agitation, so that the jet does not separate; but the liquid in it is moved by the vibration partly in a direction opposed to that of its efflux, and partly in various other directions. Hence the connexion of the parts is smaller, the jet does not form in its entire length a continuous mass, and Savart's ventral segments are produced at those places where it begins to separate.

85. If the jet issues tranquilly from a circular orifice, and if all vibrations are avoided, no, or at any rate no perceptible ventral segments are formed, even where the mass ceases to be continuous; for the liquid moves in the jet with greater velocity the longer it falls. Hence the jet becomes thinner, until the velocity in any one section is so much greater than that in the preceding, that the difference becomes too great for the force of cohesion with which the strata are held together. When the lowest stratum has separated, the separated mass moves with the velocity it has acquired by the fall, and no ventral segments are formed.

86. If vibrations are communicated to the jet after it has left the orifice, the liquid in it separates, not so much on account of the velocity which the lower sections have attained, as that a

* *Lehrbegriff der Hydrodynamik*, translated by Langsdorff, vol. ii. § 446. p. 19.

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stratum is moved upwards by the vibration, or more correctly, hindered in its descent, while the strata immediately below are either moved forward by the vibration or accelerated in their motion. Hence the separation takes place before the separated liquid has attained that great velocity, and then the ventral segments are formed which Savart has described.

This mode of separation may, under proper conditions, take place when the jet moves upwards as well as when it moves downwards, or when it moves at any angle to the horizon.

87. When a jet of water reaches the ground a noise is produced, which is sufficient, if particular precautions be not taken, to produce the Savart's segments in circular jets. If the jet be allowed to fall into a vessel made of sheet-metal, it produces a tone which still more promotes the formation of segments. If the vessel is large, and the bottom only slightly, or not at all covered with water, the tone is very deep and strong, and the ventral segments are then more prominent*. These are particularly strong when the oscillations of this vessel can be completely communicated to those of the upper one, either by joining them on the stand, or by connecting them by means of some substance which easily transmits motion.

88. Just as the continuity of the jet is diminished by the vibration of the vessel, it is also impaired when, by removing the tranquilizer, § 38, the jet assumes the spiral shape mentioned in § 80. Savart's ventral segments generally make their appearance without any tone or noise being produced in the neighbourhood, soon after the first traces of spiral motion have been perceived at some distance from the efflux orifice. The segments vanish, however, when the tranquilizer is put again in its place, because then the spiral-shaped motion in the jet ceases.

On the penetration of air-bubbles in a liquid.

89. Jets falling into a vessel which already contains some of the same liquid, cause, with but few exceptions, air to penetrate into the liquid. I made experiments several years ago, in order to find out under what conditions this penetration takes place. These experiments were communicated to the Berlin Academy of Sciences as long ago as the 8th of December, 1851, but their publication was deferred because they stand in intimate connexion with these experiments on the nature of the liquid jet.

* The changes which the tone undergoes are very surprising. The strong deep tone often changes suddenly into a much higher one, and this changes suddenly again. I have not further examined the reasons of these changes. They probably arise from the changes in the vibration of the metallic vessel when the water in it increases; the vessel out of which the water flows is set into other vibrations, and by this the form of the jet and its action on the lower vessel are changed.

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90. If a solid body which has a greater specific gravity than water be laid on the surface of water, the latter is pressed aside, but unites again immediately over the body. But if the body falls from some height into the water, the latter receives at the place where it is first met a strong push, which moves it further on one side than would be necessary to make room for the pushing body. This process is repeated in the strata immediately below the surface, and in this way a cavity is formed in the water which has a greater section than the falling body. But since the momentum of the latter in its further motion is lessened by the resistance which it experiences, at a greater depth it moves the water less on one side. Hence the cavity at a greater depth is narrower, until at length the momentum which the body has received by its free fall on the surface is destroyed, and its further sinking occurs just as if it had been tranquilly laid on the water, which is now only so much moved aside as its magnitude necessitates.

91. If the falling body, on meeting the water, possesses any considerable moving force, the cavity extends so far down that the water meets on the surface and closes before its formation below is completed. Hence air is enclosed, which afterwards reaches the surface in the form of a bubble.

92. It is scarcely necessary to mention, that bubbles are formed in exactly the same way if, instead of a solid body, single drops of water fall into the water.

The cup-shaped cavity which drops of water produce can be distinctly observed by letting them fall into water which is contained in a glass vessel, and looking at the superior stratum of liquid from the side through the water.

93. It might be believed, that a cavity whose section is greater than that of the falling drop or solid body, was not necessary for the production of air-bubbles; for if the water did not separate further than the section of the body requires, air would also be enclosed, if this body only moved quickly enough so that it might have reached a sufficient depth below the surface before the water above had joined. It is easy to show, that, if this were the case, the quantity of air would be far too small to produce the bubbles which are actually observed. For if small solid bodies, for instance shot, be let fall into a vessel which contains a stratum of water 2 centims. high, then if the shot fall from a height of $1^{m}{\cdot}25$, air-bubbles are obtained whose contents are many times greater than those of a cylinder of the height of the water and the diameter of the falling shot. This phænomenon is more surprising when peas, instead of shot, are used. It is only necessary to think of the great bubbles which are formed by rain-drops falling almost vertically into shallow masses

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of water, to see that these bubbles must be formed from a cavity whose magnitude is far greater than that of the drops.

94. The greater the force with which the falling body meets the water, the greater is the cavity, and hence the bubble resulting therefrom. If solid bodies, as peas or shot, fall from a few inches' height into the water, very small air-bubbles are obtained; these are much larger when the bodies fall from the height of a few feet.

95. But even if the water-drops fall into the water from a height of many feet, the air-bubbles produced go only to a very small depth, at most a few inches below the surface. Even if, instead of water-drops, peas be used, the result is quite similar; there remains then a small air-bubble adhering to almost every pea, which goes with it slowly to the bottom.

96. But if leaden bullets as large as, or larger than, shot be let fall into the water, the bubbles are seen to reach to a greater depth. If musket-bullets be used for this experiment, and if they be allowed to fall into glass vessels two or three feet deep, then along the whole of the way through which the bullet passes in the liquid, the air is seen to escape in bubbles, the last and largest of which is separated when the bullet strikes against the bottom.

97. Just as lead causes the penetration of air to a great depth, so can this be effected by single separated masses of water when they fall on the water in rapid succession; for each successive drop pushes anew, producing a stronger motion, and hence one extending deeper. That is the reason why air-bubbles formed by single drops of water only reach to a considerable depth when they form a continuous, or almost continuous jet.

98. For even when a jet is entirely continuous, it carries with it air-bubbles downwards. In a previous treatise, "On the Motion of Liquids*," I have mentioned in § 18 that a cavity could be perceived when such a continuous jet fell vertically on the surface of the water. The first origin of this is doubtless similar to that of the falling of a solid body into water, but its continuance has an entirely different reason; for so long as the cavity is there, the jet only meets the water at the deepest part of it. It is there pressed towards the side; is not, however, moved in a horizontal direction, but is pressed upwards by the resistance which the water present offers. And since this action of the resistance is repeated throughout the whole distance, the motion becomes curvilinear, and there is formed a curved surface or cup-shaped cavity.

99. From this explanation, it is shown that this cup-shaped cavity can only be formed under a certain velocity of the jet; for if the velocity be small, the water is moved aside with only

* Poggendorff's *Annalen*, vol. lxxx. p. 1. Phil. Mag. for January 1851.

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moderate force, and only deviates as far as the section of the falling jet requires. The water can then draw itself up the jet, as Prof. Tyndall* has observed, just as in a glass rod. But if a sufficiently strong pressure be employed, and the cup-shaped cavity is produced, it is manifest that it can only remain so long unchanged as the water in which it is produced does not disturb its form by other motion. As soon as this is the case, the air which it encloses is carried down with the water into the liquid.

100. If a water-jet from a perfectly circular aperture 3 millims. in diameter, and under a pressure of a column of water 2 or 3 metres high, be allowed to fall vertically on the smooth surface of water contained in a vessel $0^{m}{\cdot}6$ in height and width, and if care be taken that the jet meets the surface (which must be only a few centimetres from the efflux orifice) when the former is quite clear and transparent, the above-mentioned cavity can be distinctly, and for some time seen. But after some time the water in the vessel into which the jet falls always begins to move. This motion is very perceptible in particles of dust or very fine bubbles which float on the surface. They are seen to move slowly in a circle about the place where the jet meets the water. This rotation becomes more rapid, and at a certain velocity a cavity is formed which is drawn downwards in the liquid in screw fashion, and carries with it innumerable small air-bubbles.

101. The formation of this screw-shaped cavity is promoted by setting in rotation, in any other manner, the liquid into which the jet falls. If this be done about the place where the jet meets the surface, the cavity winds like a screw either to the right or left, according as the rotation has the one or the other direction.

102. As this screw-shaped cavity can be formed by setting the water in rotation, so it can be stopped by preventing the rotation. If a solid plane, as a thin board or a metal sheet, be held vertically in the water into which the jet falls, so that one of its edges is parallel to, but at a small distance from, the axis of the jet, the screw-shaped cavity is not formed, or if already formed, it disappears.

103. In order to be able to produce or stop this rotation more certainly, I made use of the tranquilizer described in § 38. If this be so placed in the water that the jet falls inside the space *abcdfg*, fig. 13, the rotation is prevented, and no screw-shaped cavity is formed. But if the tranquilizer be rapidly turned about the prolongation of the jet, and the water be thus set in rotation, the cavity is formed immediately, and lasts as long as the rotation, but disappears as soon as the apparatus is held firm.

104. If, instead of the tranquilizer, the water be moved in any other way, it is only seldom that it rotates exactly about the pro-

* Phil. Mag. S. 4. vol. i. p. 105.

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longation of the jet. It is generally so moved that it has no determinate axis of rotation. The cavity just formed is seen to move with the water from its place of formation, so that it is observed on the surface at the same time that it swims in the water. This phænomenon is particularly seen when the water is only a few inches deep.

If the water be moved quite irregularly, the cavity is seldom produced.

105. It is easy to see how a screw-formed cavity is produced by a rotation of the water. In consequence of the centrifugal force, the particles move away from the axis of rotation in all directions; but they experience a greater resistance in the deeper strata, where the pressure of the water is greater. Hence their distance from the axis of rotation is less in the lower strata, and diminishes with the depth. In this manner a funnel-shaped space is formed, in which the centrifugal force acts in an opposite direction to the hydrostatic pressure. But since, in the different strata, the rotation does not take place readily about the same axis and with the same velocity, the cavity assumes a screw shape.

106. If water which does not rotate, but is quite tranquil, is met by a jet, it produces, as was mentioned, § 100, a rotation after some time. This is not produced as long as the jet is quite regular, not even if a jet from a square or other polygonal orifice be used. But the slightest irregularity in the jet, and this is much more the case in jets from angular than from circular apertures, sets the water near the place where the jet meets it in irregular motion. The resultants of the motions of the individual parts do not go through the same point, and hence a rotation commences which rapidly increases when it has once begun. It is scarcely necessary to mention how easily such an irregularity occurs in circular jets, or rather how difficult it is to avoid them. Any motion communicated either to the vessel from which the water flows, or to that into which it flows, is sufficient to produce it, for the motions produced are easily communicated from the one to the other.

107. If the jet meets the spiral-shaped windings of such a cavity, which is changed by a motion of the surrounding water, the wall of the cavity is hit by the jet, air is enclosed and carried down with the water which streams in. The same occurs if the cavity changes its form or position by an alteration in the nature of the jet.

108. If the water which supplies the jet be set in vibrations, the jet assumes, even if it comes from a circular aperture, the round form mentioned, § 80, and then air-bubbles penetrate into the water very easily. The force with which this happens

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depends on the strength of the spiral winding of the jet, as well as on the pressure with which it acts on the liquid.

109. If an aperture of 3 millims. diameter in a thin wall be so used that the pressure under which the water issues can be increased at pleasure to the pressure of a column of water $2^{m}\cdot5$ high, then if the jet under a small pressure meets the surface at the distance of a few centimetres from the orifice, no bubbles penetrate into the water, even if it be set in motion or rotation in any other manner; but if the pressure is increased, the jet changes, the spiral-shaped cavity is formed, and air-bubbles penetrate into the liquid. If the rotation of the water be then prevented by means of the tranquilizer, no air-bubbles penetrate, even under the application of a pressure of $2^{m}\cdot5$. But if the efflux aperture be changed for a wider one, for instance of 9 millims. diameter, the jet sets the water in such a whirlpool-shaped motion that the tranquilizer has no effect, and it is impossible to prevent the penetration of air-bubbles.

110. If the water be met by the jet when it is no longer continuous, air always penetrates; but since the place where the jet ceases to be continuous depends on the vibrations communicated to the vessel from which it flows (§ 86), and since such vibrations may be produced by the most insignificant circumstances, it may easily occur, that, while everything seems unchanged, air-bubbles suddenly penetrate. I will mention an experiment very easy to perform, but which is very surprising.

111. If from a vessel of moderate size (that used was about $0^{m}\cdot3$ in diameter and height, and stood upon a not very firm frame) a jet be allowed to flow through an orifice 8 millims. in diameter, and if it be caught in a glass held in the hand, the bubbles are first seen, if the glass is gradually lowered, where the jet ceases to be continuous. If the glass be held at a somewhat higher place where no air-bubbles penetrate, and if, when the first glass is full, the excess of water be caught in a second, likewise held in the hand, no air-bubbles are seen to penetrate into the water of the first glass. If the water from the second be let fall into a tin vessel standing on the ground, a noise is produced, and air-bubbles penetrate into the first glass. Hence by an easy movement of the second glass, which is quite separated from every other glass, air can be made to penetrate or not at pleasure into the first, which remains fixed in its place. It is very surprising to see bubbles making their appearance in the first glass as soon as a small quantity of water falls into the lowest tin vessel, and disappearing again as soon as the falling of the water ceases.

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Jets which contain air in their interior.

112. Besides the cases already mentioned, in which air penetrates with the jet into the water, there is one case essentially different from these; for if the water in the vessel from which the jet issues begins to rotate, which easily happens when no tranquilizer is employed, there is formed after some time a funnel-shaped cavity. This often draws itself downwards, not only to the efflux orifice, but even, when this is not too small, beyond the orifice into the jet, which hence assumes a peculiar appearance. If it issues from a circular orifice, and is without dilatations, the air often draws itself down in it, changing the jet into a tube, which becomes narrower with increasing distance from the aperture; but if the jet possesses dilatations, or spiral-shaped windings, § 80, these appear filled with air, and it has then the appearance of a spiral-formed hollow tube. When the jet issues from a polygonal aperture, air is only drawn in, if it has already assumed a spiral-shaped form in consequence of rotation. This is seldom regular, and becomes more irregular by drawing air in. Hence the form which it assumes is difficult to determine, and it can only be said in general terms to be similar to a spiral hollow jet from a circular orifice.

113. If the hollow jet meets the surface of perfectly tranquil water, the air contained in the jet does not penetrate far below the surface as long as the motion is quite regular; but after a short time the motion becomes irregular, rotation ensues, and now the air goes down in little bubbles to a great depth with the water.

114. After this discussion of the different conditions under which air penetrates into water, the so-called water-bellows, which I have discussed in the appendix to a previous treatise "On the Motion of Liquids," needs no further explanation. For it is manifest, either that the air, by a funnel-shaped motion of the water, gets into the jet, and is carried down with this into the water of the pipes, or that the jet, without containing air, is so set in motion where it reaches the water, that air is enclosed and carried down with the water rushing in.

115. Looking at the preceding investigations, it will be conceded that they give a clearer insight into the phænomena of efflux. The remarkable forms of jets are explained on the simple laws of motion, of resistance, and of cohesion. The influence which a motion in the vessel from which the jet issues exercises upon its form, and the manner in which this influence is exerted over the whole length of the jet, are so far explained that it is possible to produce at pleasure all the different forms of jet. And not only this, but the reverse also: to determine from the

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form of the jet its origin; and to judge whether any and, if any, what irregularities are in the vessel. The conditions also for the penetration of air into the jet are laid down; the contraction of the jet is fully discussed, and the differences in its form from a square or a circular aperture are shown; so that I believe I may assume that these investigations will not be without success in determining the velocity of efflux.

XXIV. *On the Crystalline Form of Rhodonite.*

By R. P. Greg, *Esq.*, *F.G.S.**

Siliciferous Oxide of Manganese, or *Rhodonite*, is described by W. Phillips (see his 'Mineralogy,' 4th edition, published by R. Allan) as having for its primary form a doubly oblique prism, with cleavage apparent in two directions perpendicular to each other, that parallel to P highly perfect; say MT=121° 0′, TP=112° 30′, MP=93° to 94°.

Dufrenoy, in his 'Mineralogy,' states that this mineral has four cleavages; two of about 87·5, and two others perpendicular to these, and that it has an oblique rhombic prism for its primary.

Rose, Dana, and Brooke and Miller in their recent edition of Phillips's 'Mineralogy,' agree in stating that rhodonite is isormorphous with augite, having three cleavages: viz. *a*, perfect; *b*, less perfect; *m*, imperfect;

ab	=	90°	0′
mm′	=	92	54
ma	=	136	27
bc	=	90	0

That rhodonite is isomorphous with augite has perhaps been too hastily assumed to be the case, arising from the coincidence of each mineral giving cleavages of about 87° 5′, as well as from a similarity in their chemical formula; rhodonite having for its formula $3MnO\,2SiO^3$, where part of the MnO is replaced by FeO, ZnO, CaO, MgO; while augite is represented by $3RO\,2SiO^3$, where RO consists essentially of MgO and FeO.

Hitherto perfect crystals of rhodonite, having also brilliant faces, have been quite a *desideratum* among mineralogists. I have, however, very lately received specimens from the Paisberg iron mine near Phillipstadt, in Sweden, on which are implanted some very perfect and brilliant crystals, from the examination of which I have been enabled, I think, without doubt to make out its true form and principal cleavages, and to give correct angles.

The following form, as given in the figure, I have observed to

* Communicated by the Author.

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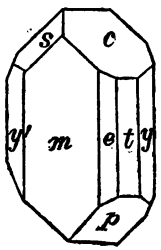
be a constant one in the Paisberg crystals; the face *s* is frequently, as well as *m*, a predominant one:—

mt	= 87°	20′
tp	= 86	10
mp	=110	40*
me	=136	20
my′	=138	20
ms	=148	42
mc	= 86	35
tc	=142	30



Cleavage highly perfect, parallel to *m* and *p*, less so to *t*. From this it would appear that rhodonite, or manganspath, belongs to the anorthic system, having, as W. Phillips described it, a doubly oblique prism for its primary; it cannot therefore be isomorphous with augite, though having a similar formula. That rhodonite has any regular cleavage giving an angle of 90°, appears to me to be very problematical; the figure and angles I have now given may, however, afford some key or explanation to the various and discordant descriptions hitherto published respecting the form and cleavages of this mineral.

XXV. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D.*

[Under this title it is proposed to give, from time to time, abstracts of the more important chemical contributions to the foreign scientific journals. The chemist will not find in them a substitute for the use of the original memoirs or their translations, as they will merely give the final results, and not detailed descriptions of the processes. Their object is to afford to the general scientific reader an idea of the progress and direction of the labours of foreign chemists.]

LIEBIG'S *Annalen der Chemie und Pharmacie* for September contains two important papers by Liebig. The first of these is "On the Constitution of the Compounds of Mellone." The author's formula for these compounds had been called in question by Gerhardt, who assumed in them a radical hydromellone. Liebig's earlier formula for the mellone metals was

* This angle appears to vary slightly measured on cleavage faces, viz. from 110° 30′ to 112° 30′, W. Phillips giving 112° 30′; the crystalline variety from Paisberg, 110° 30′ to 111° 0′; and that from Franklin, New Jersey (showing also occasionally the faces *m*, *e*, *t*) 111° 30′.

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$C^{12} M^2 N^8$, and Gerhardt represented them as $C^{12} H M^2 N^9$, and the radical hydromellone as $C^{12} H^3 N^9$.

The latter view is quite incorrect. After a careful study, the author had found that in the formation of mellonide of potassium the materials, sulphur and dried ferrocyanide of potassium, united to form sulphocyanide of iron, and that the mellonide of potassium resulted from the decomposition of this body at a very high temperature. The formation, under these circumstances, of a hydrogen compound from materials which were free from it was impossible. Further, in the analysis of hydrogen compounds, there is always an excess of water found, but in all the very careful and accurate analyses of mellone compounds, there had never been more than half the quantity of hydrogen found which Gerhardt's formula requires. Some criticism is devoted to Gerhardt's views of the products obtained by Henneberg in the decomposition of mellonide of potassium by alkalies, and in the other compounds derived from mellone. Liebig charges Gerhardt with a tendency to reject formulæ derived from accurate experiments on insufficient grounds, merely because they do not favour his personal opinions.

Henneberg's investigation of the decomposition of mellonide of potassium led the author to resume the study of the subject.

If the author's old formula for mellonide of potassium were correct, it must give on decomposition, along with other products, formic acid. But careful experiments showed that under no circumstances was this acid a product of decomposition, and the old formula must be rejected. After much investigation results were obtained which led to the unexpected formula $C^{18} N^{13} H^3$ for hydromellonic acid.

The acid was obtained by precipitating corrosive sublimate with mellonide of potassium, dissolving the mellonide of mercury formed in hydrocyanic acid, precipitating the mercury by sulphuretted hydrogen, filtering, and expelling the hydrocyanic acid by gentle heat.

In the acid either one or all the equivalents of hydrogen are replaceable by metals. As mellonide of potassium is the best source for the other compounds, the author devoted a good deal of attention to its preparation, and goes into the particulars of the different processes in some detail.

A great many analyses were made of the different salts. There are three potash salts, a neutral one, whose formula is $C^{18} N^{13} K^3 + 10HO$; an acid insoluble salt, $C^{18} N^{13} K^2 H$, and an acid soluble one, $C^{18} N^{13} K H^2 + 6HO$. The silver salt has the formula $C^{18} Ag^3 N^{13}$. Numerous accurate analyses of the dry salt, effectually disproved the idea of Gerhardt that the radical contains hydrogen.

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Henneberg found that mellonide of potassium treated by strong potash gave cyameluric acid, for which he proposed two formulas, one of which contained an equivalent of hydrogen. Gerhardt assumed this formula to be correct, and found in the production of cyameluric acid an objection to Liebig's formula for mellone. The analyses which Liebig made, gave in no case more than one-third the quantity of water which an equivalent of hydrogen in the radical requires. The formula of the hydrated acid is $C^{12} N^7 O^6 H^3$, and of its salts $C^{12} N^7 O^6 M^3$. Gerhardt had assumed $C^{12} N^7 O^6 H^4$ to be the formula for the anhydrous acid, and for the salts $C^{12} N^7 O^6 H M^3$.

Liebig's second paper is "On a new Cyanic Acid." When fulminate of mercury is boiled for a long time in water, it changes its colour and crystalline structure, being converted into a greenish-brown powder, and loses, in a great measure, its fulminating properties. In this process a new acid is formed, which Liebig calls *fulminuric acid.* It stands in the same relation to fulminic acid as cyanuric acid to cyanic acid. But while cyanuric acid is tribasic, this is monobasic, that is, saturates one equivalent of base. The formula of the acid dried at 100° C. is $C^6 N^3 H^3 O^6$, and its salts contain in the place of one equiv. H an equivalent of metal. Its formula is identical with dry cyanuric acid, but the properties of the two acids are quite distinct. He gives a new process for preparing fulminate of mercury, which affords it pure and in large quantity. It only differs from the process usually adopted in the proportions of the materials, and in the manipulatory details.

Fulminurate of ammonia forms brilliant white crystals which have a refractive and dispersive power equal to that of bisulphide of carbon. It also exhibits the phænomena of double refraction. The potash salt has the same properties. Besides these, the baryta, silver, lime, magnesia and lithia salts were investigated.

Simultaneous with, if not prior to the appearance of Liebig's paper, a communication had been made to the *Bulletin de St. Pétersbourg* by M. Schiskoff, on isocyanuric acid. This acid is identical with the fulminuric acid, and is prepared by the same process. Schiskoff has gone into the subject at greater length than Liebig.

Wicke gives an "Analysis of some infusorial Earth found in the Lunebourg Heath," which shows it mainly to consist of silica. The same chemist recommends, in the preparation of molybdate of ammonia from molybdate of lead, that the ore be treated with sulphuretted sulphide of ammonium. A double salt of sulphide of ammonium and sulphide of molybdenum is formed, from which molybdic acid may be easily obtained.

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Bolley gives, in a paper "On the Molecular properties of Zinc," a *résumé* of less known observations on the physical properties of that metal by other chemists, in addition to experiments of his own. Zinc is generally stated to have a crystalline lamellar structure, but the author had found that this was only the case with zinc which had been heated to almost a red heat, and then cooled; while that which had been simply melted and then cooled, had always a small granular structure. The differences in the observations on the spec. grav. of zinc are greater than in any other metal; they vary from 6·86 to 7·2. Bolley ascertained by actual observation on various specimens, that there existed hollow spaces in the interior of the zinc, which were amply sufficient to account for these variations. He cast small cylinders of zinc about 10 grms. in weight, using every precaution to prevent air being enclosed. These pieces were divided and subdivided, and the spec. grav. taken, in small pieces until well-agreeing results were obtained. It was found, the smaller the pieces, the higher was the specific gravity. He found that zinc which had been heated to melting, and then quickly cooled, had a specific gravity of 7·178, and when slowly cooled 7·145; that which had been heated to redness and quickly cooled, of 7·109, and slowly cooled, 7·120. Zinc which had been simply melted was comparatively malleable, while that which had been heated to redness was not at all so. The ease with which commercial zinc dissolves is ascribed to the presence in it of foreign metals. But even in pure zinc there are differences in the solubility, which arise from the temperatures to which it has been heated. Pure zinc was heated to melting, one part poured in cold water, and another poured on a warm plate. Another specimen of the same zinc was heated to redness, and cooled by the same method. It was found that the latter was far more easily dissolved in dilute acids than the former. He concludes that zinc melted at as low a temperature as possible is distinguished by—1. granular fracture; 2. probable higher specific gravity; 3. greater malleability; 4. less solubility in dilute acids; while that melted at a higher temperature has—1. crystalline lamellar fracture; 2. probable less specific gravity; 3. greater brittleness; and 4. far greater solubility in dilute acids.

Bolley suggests that zinc is dimorphous. He finds a support for this idea in the fact that the atomic volume of zinc is very near that of platinum, iridium and palladium, three metals which are dimorphous.

In the October Number of the same Journal Schlossberger has two papers on Physiological Chemistry. An investigation of the uterine milk of the Ruminantia showed him that it contains

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no fibrine or sugar, but a considerable quantity of albumen. The ash was composed of phosphoric acid, lime and the alkalies, with traces of chlorine and oxide of iron. The secretion was found to be the same in the fœtus of six and of twenty weeks' age. The nourishment of the fœtus is far poorer in respiratory elements than the nourishment of the new-born animal, but on the other hand, it is richer in plastic elements.

The stomach of the fœtus was found to contain a liquid consisting mostly of liquid mucous matter (Scherer), but in which was no albumen, while the amniotic liquid contained much albumen. The stomach of the fœtus has the property of turning milk sour.

The same chemist instituted experiments as to whether milk becomes sour by remaining a length of time in the lacteal glands. The experiments were made on a woman, and on a cow. He found that in a normal state of health it did not become sour, even on standing several days.

Wöhler found that picric acid, treated with protoxide of iron, gave a new acid which he named *nitrohæmatic acid.* Pugh has found that this acid is identical with picramic acid, produced by the action of sulphuretted hydrogen on picric acid.

De Luna made some experiments on the possibility of substituting for sulphuric acid some of its compounds, in cases where they are to be had cheap. He found this to be the case with the sulphate of magnesia which occurs in the province of Toledo. By heating this body with common salt, hydrochloric acid is evolved, and a residue left, consisting of magnesia and sulphate of soda. From this residue a sulphate of soda of greater purity than the commercial salt is easily prepared. In the above process, by adding manganese, chlorine may be obtained.

Similarly, nitric acid is formed on heating sulphate of magnesia and nitrate of soda or potash.

In connexion with his investigations on the anilide compounds of tartaric and pyrotartaric acids, Arppe has examined the corresponding compounds of malic acid. By heating a mixture of 2 equivs. of aniline and 3 equivs. of malic acid, two bodies are formed. The first, malanilide, contains the elements of 1 equiv. aniline and 1 equiv. malic acid minus 2 equiv. water $= C^{12} H^7 N + C^4 H^3 O^5 - 2HO = C^{16} H^8 NO^3$. The second contains the elements of 1 equiv. aniline and 2 equivs. acid minus 4 equivs. water $= C^{12} H^7 N + C^8 H^6 O^{10} - 4HO = C^{20} H^9 NO^6$.

To separate these, the brown mass resulting from the decomposition was treated with water, which dissolved out the malanile. The residue, which is malanilide, after solution in hot alcohol and purification with animal charcoal, crystallizes

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out in thin colourless laminæ, which melt at 175°, and sublime at a higher temperature without much decomposition.

When malanilide is boiled with concentrated solution of potash, a remarkable change takes place. The greater part of it is taken up by the potash, and at the same time a slimy body is formed which floats on the surface. On the addition of water, the substance, which is partially dissolved in the potash, is separated out as a white insoluble powder. By repeated washing with water this is obtained free from potash, and may then be crystallized from alcohol. The form, analysis and properties of this body prove it to be *tartanilide*, $C^{16} H^8 NO^4$. This only differs from malanilide by containing one atom more of oxygen, but the change produced by the potash is not so simple as the formation of the slimy body shows.

The solution of the malanile is slightly evaporated, and any dissolved malanilide is separated by filtration. It is then purified by treatment with animal charcoal and crystallization. It crystallizes easily, and in forms which vary with the concentration of the solution. It melts at 170° C. When boiled with aqueous ammonia it is changed into *malanilic acid*, which forms with the ammonia a heavy crystalline salt. The acid is separated by treating the ammonia salt with baryta water, and carefully decomposing the baryta salt formed with sulphuric acid. The slightest excess of sulphuric acid reconverts it into malanile. It crystallizes in small grains, expels carbonic acid from its salts, forming with the bases salts which are mostly soluble. Its formula is $C^{20} H^{11} NO^8$, *i. e.* 2 equivs. malic acid and 1 equiv. aniline, minus 2 equivs. water.

By heating malanile with nitric acid, the nitro-compound is formed, but so mixed with a resinous matter that it is impossible to separate them.

Hofmann and Muspratt found that when dinitrobenzole is treated with sulphuretted hydrogen, paranitraniline is formed with separation of sulphur,

$$C^{12} (H^4 NO^4) NO^4 + 6HS = C^{12} (H^6 NO^4) N + 4HO + S.$$

In treating dinitrobenzole by ammonia and sulphuretted hydrogen, Arppe observed that the reaction was more complicated, for in addition to sulphur there separated out hyposulphite of ammonia, and a small quantity of an organic sulphur compound which appears to be a weak base.

This body may be obtained by dissolving out the hyposulphite of ammonia with water, and the sulphur with bisulphide of carbon. It is difficultly soluble in hydrochloric acid, from which it is easily separated by alkalies. It is best purified by treatment with excess of strong SO^3, which dissolves it, and from

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which it is separated by washing with water. It is an amorphous powder, and does not readily form salts; is little soluble in alcohol and water, as also in chloroform and æther. It is a product of decomposition of paranitraniline, as Arppe found by direct experiment. A pure specimen prepared in the above manner from paranitraniline gave analytical results which led to the formula $C^{12} H^8 N^2 S^2 O^4$. Arppe has named it *nithialine*.

With none of the bases formed by Zinin's process does this substance, or any analogous one, seem to be produced, except with paranitraniline, as direct experiment showed.

Hlasiwetz gives a preliminary account of two new bodies which he has obtained from phloretine. This body is boiled with strong potash, the solution evaporated, and the excess of potash removed by carbonic acid. If the liquid be then further evaporated and treated with alcohol, the potash salt of a new acid is obtained, which he has named *phloretic acid*. It is easily purified, possessing a great tendency to crystallize. It expels CO^2 from its salts, uniting with the base to form easily crystallizable salts. Of these the baryta and zinc salts are the most beautiful. It has the same deportment with reagents as lichenic acid. Its formula is $C^{18} H^{10} O^5$, HO. It is monobasic; the formula of its salts is $C^{18} H^{10} O^5$, MO.

The salt residue from which the phloretate of potash had been dissolved out, and the carbonate of potash, contain an interesting neutral body. This is obtained by treating the solution of the residue in water with dilute sulphuric acid, evaporating again and treating the mass with alcohol. The alcoholic solution is again evaporated, and the residue crystallized out of water. Its most remarkable property is its sweetness, from which it has been named phloroglucine. It has the greatest resemblance to orcine, and gives, like it, a bromine substitution compound.

The formula of the body crystallized from water is $C^{12} H^{10} O^{10}$, for that crystallized from æther, $C^{12} H^6 O^6$. The bromine compound contains 3 equivs. hydrogen, replaced by bromine, and is, in its properties, very similar to bromorcëid.

As by the decomposition of phloretine by alkalies no other body is formed, it may be expressed thus:—

$$C^{30} H^{15} O^{10} + KO\,HO = C^{18} H^{10} O^5\,KO + C^{12} H^6 O^6.$$

These bodies stand evidently in a very near relation to the constituents of lichens.

The same author makes a communication, that direct experiments have convinced him that quercitrine and rutinic acid are identical, and have the formula $C^{36} H^{18} O^{21}$. The discrepancies exhibited by some of the analyses of the latter body with the

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formula of quercitrine, are explained by the difference in the quantity of water.

Buchner recommends for the purification of sulphuric acid from arsenic, the addition of common salt. The hydrochloric acid evolved unites with any arsenious acid present to form terchloride of arsenic, which is easily expelled at a gentle heat.

XXVI. *On the Determination of Bismuth by Weight and by Volume.* *By* R. West Pearson *of Manchester**.

THE method at present employed for the determination of bismuth, consists in treating the solution of the metal with sulphuretted hydrogen, filtering off the precipitated sulphide of bismuth, which, after washing with water, is decomposed by digestion with nitric acid. Nitrate of bismuth is thus formed and sulphur liberated. The sulphur is removed by filtration; the oxide of bismuth is then precipitated from its solution as carbonate of bismuth: upon the first addition of alkaline carbonate a considerable quantity of the precipitate produced is redissolved. A. Stromeyer† states that the precipitate by carbonate of alkali is somewhat soluble in excess, but precipitated by caustic alkali. According to L. Laugier‡, the precipitate is completely soluble in carbonate of ammonia, partly so in carbonate of soda, and insoluble in carbonate of potash. According to Berzelius§, however, oxide of bismuth is not soluble in carbonate of ammonia unless phosphoric acid or arsenic acid is present. Rose‖ directs, that to obtain that portion of bismuth which remains in solution after exposure to heat and air, it is necessary to repeat the operations just indicated, *i. e.* precipitate as sulphide of bismuth, obtain in solution as nitrate, precipitate as carbonate of bismuth, and expose at a warm temperature to the atmosphere for several hours; after which convert the carbonate by ignition into oxide of bismuth, in which state weigh. This method, it is evident, is open to a serious objection in the great amount of time consumed in its execution. It is also inapplicable to solutions containing lead or cadmium. Hydrochloric acid and soluble chlorides must also be absent from the solution.

R. H. Brett¶ found that carbonate and oxide of bismuth

* Communicated by the Author.
† Poggendorff's *Annalen*, vol. xxvi. p. 553.
‡ *Ann. de Chim. et de Phys.* vol. xxxvi. p. 332.
§ *Jahresber.* vol. xii. p. 166.
‖ *Handb. der Analyt. Chemie*, vol. ii. p. 145.
¶ Phil. Mag. vol. x. p. 95.

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recently precipitated, were readily dissolved by chloride of ammonium. The same remark applies to the solvent action of chloride of calcium.

For the volumetric determination of bismuth no method has hitherto been proposed. To supply the desideratum which thus exists in analysis, viz. a neat, expeditious, and accurate method for the determination of bismuth, I began a series of experiments, and in the present communication I have to describe a method which I find to yield satisfactory results. It is based upon the fact that chromic acid, when in liquid contact with bismuth, combines in constant proportions to form an insoluble compound, definite in composition, which is produced under all circumstances. The value of this reaction as the basis for a volumetric mode of analysis, depends upon the characteristic colour of chromic acid and its compounds. I shall proceed to describe, in the first place, the results of some experiments on the nature of the reactions upon which the method is based; secondly, notice the estimation of bismuth by weight; thirdly, its determination by volume; and lastly, adduce the combining ratio of chromic acid and oxide of bismuth. The agent I propose to employ, bichromate of potash, has hitherto never been suggested for that purpose.

Chromate of potash, KO, CrO^3, precipitates, on addition to solutions of most metals, chromates corresponding to the formula MO, CrO^3. Bichromate of potash, $KO, 2CrO^3$, combines by double affinity with metallic oxides, as in the case of simple chromate of potash; the former giving rise to bichromate compounds of the formula $MO, 2CrO^3$, which are, without exception, soluble in water.

A peculiar combination of bichromate of potash takes place on its addition to salts of bismuth, viz. the formation of a simple chromate of bismuth, $BiO^3\,CrO^3$, in which each equivalent of chromic acid is united with one equivalent of oxide of bismuth. I found upon inquiry, that the phænomena of the two equivalents of chromic acid contained in that of bichromate of potash, combining separately with single equivalents of other bodies, took place among the metals with lead and bismuth, and among the alkaline earths with baryta only. I invariably found that acid solutions of other bodies remained perfectly clear upon the addition of bichromate of potash. By means of this agent, therefore, bismuth, lead, and baryta may be separated singly, or together, from all other bodies when in solution. I also found that while chromate of baryta is soluble in dilute nitric acid, the chromates of lead and bismuth are comparatively insoluble. The solubility of chromate of bismuth in water, acetic acid, nitric acid, and caustic potash, I determined quantitatively. I immersed a quan-

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tity of well-washed chromate of bismuth in these liquors for twelve hours with frequent agitation of the mixture. To determine the quantity of bismuth dissolved from the tints produced in the several solutions by sulphuretted hydrogen, I prepared dilute standard solutions of nitrate of bismuth, containing respectively known quantities of metallic bismuth. Through each of these solutions I passed sulphuretted hydrogen gas, and in this way obtained "comparison tests" of bismuth. When examined by this method I found the following to be the—

Per-centage Solubility of Chromate of Bismuth in water, acetic acid, nitric acid, and potash.

	In water.	In acetic acid.	In nitric acid, sp. gr. 1·03.	In potash, sp. gr. 1·33.
Chromate of bismuth	·00008	·00021	·00024	·00016

I found by experiment that the following reaction takes place with bichromate of potash and nitrate of bismuth:—

70,000dth gr. of bismuth	:	precipitate immediate.
100,000dth	...	opalescence produced.
150,000dth	...	reaction ceases.

The characteristic colour of chromic acid is possessed in a remarkable degree by the chromates of potash. Thompson states that one part of chromate of potash may be recognized in 40,000 parts of water. I found that bichromate of potash, when diluted 70,000 times, may be detected by its yellow colour.

Estimation of Bismuth by Weight.

Previous to precipitating the bismuth in solution, as chromate, it is necessary, as when precipitating by carbonate of ammonia, to ascertain the absence of lead. This may be effected, and, if present, its removal accomplished, by processes to be indicated further on. Nitric acid is the most convenient solvent for ores, &c. of bismuth, taking care to remove any large excess of acid in solution by evaporation. In the absence of lead the solution is supersaturated with bichromate of potash, and the mixture warmed to aggregate the precipitate, which is collected upon a filter and thoroughly washed with water. When washed, the precipitate and filter are dried. The chromate of bismuth may now be estimated after combustion of the filter, or weighed in a filter of known weight; deducting the latter from the total obtained, 1 grain of bismuth is contained in 1·49074 grain of the chromate of the oxide.

Separation of Bismuth from Lead and Baryta.—I include baryta, since on the removal of lead and baryta, bismuth may be

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estimated without further manipulation in presence of all other metals or earths. In systematic analysis we should have to deal with lead only. I have found three methods available for the separation of lead and baryta from bismuth.

I. Add to the solution containing, amongst other substances, lead, bismuth, and baryta, dilute sulphuric acid in excess. Sulphates of baryta and lead will precipitate, the mixture is filtered and the insoluble sulphates drenched with water. The filtrate which contains the bismuth in solution as soluble sulphate is evaporated to expel excess of acid, bichromate of potash is added in excess, and the experiment proceeded with as above.

II. If necessary, add a slight excess of nitric acid to the solution containing lead, bismuth, baryta, &c., and precipitate with bichromate of potash; on filtering, baryta will remain in solution soluble in nitric acid. The precipitated chromates of lead and bismuth are now treated with caustic potash of spec. grav. 1·38. Chromate of lead is readily and completely dissolved by caustic alkali, while chromate of bismuth is comparatively insoluble. By treating the chromates of lead and bismuth with potash, therefore, we obtain the lead chromate in solution, and by filtration separate it from the bismuth chromate which is insoluble. From the amount of chromate of bismuth obtained, the metal is calculated as in I.

III. That oxalic acid and neutral oxalates, on addition to solutions of bismuth, precipitate oxalate of bismuth, is a fact long known. On adding an excess of oxalic acid with a slight elevation of temperature, I have observed that the precipitate produced on the first addition of acid is readily redissolved. This circumstance has not hitherto been noticed, I believe. Oxalate of lead is completely insoluble in excess of acid, even in a hot solution. If, therefore, we supersaturate a solution containing lead and bismuth with oxalic acid, and heat to ebullition, the bismuth remains in solution, while the lead remains as an insoluble compound. By means of oxalic acid, I find that lead and bismuth may be separated with facility and accuracy.

Of these three methods the first is generally applicable, especially when the bismuth alone is to be estimated. The second method is particularly adapted when the lead or baryta are to be estimated also. The third process enables us to separate lead and bismuth with great accuracy. Circumstances will suggest a preference.

Separation of Cadmium from Bismuth.—These two metals when associated cannot be separated by any known method with facility. By the use of bichromate of potash, bismuth may be precipitated perfectly free from cadmium when the two metals are in solution.

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In a solution containing . . .	Cadmium	3·765 grs.
	Bismuth	1·244 ...
I obtained, taking the cadmium by difference	Cadmium	3·76517 grs.
	Bismuth	1·24383 ...

The bismuth was estimated by volume.

Estimation of Bismuth by Volume.

The mode of procedure in estimating bismuth by volume analysis is exactly the same as in the now common methods of volumetric determinations. A graduated solution of bichromate of potash is added to a colourless solution of bismuth until the metallic oxide in solution is converted into insoluble chromate of bismuth. By observing the effect of continued addition of the bichromate salt, it is easy to note the rise of a deep yellow colour in the supernatant liquid. This indicates an excess of bichromate of potash, and of course the saturation of the liquid.

Preparation of standard solutions.—Bichromate of potash, the agent to be employed, as met with in commerce, is usually contaminated with sulphate of potash and chloride of potassium. The removal of these and other impurities may be effected by repeated crystallization of the bichromate salt.

7·135 grains of pure crystallized bichromate of potash are weighed off and dissolved in 1000 grains of water in a large graduated test mixer or other convenient vessel. To avoid subsequent repetition, I shall call this solution the "bichrome test," and to distinguish from similar ones affix the letter A—bichrome test A. A second solution, one-tenth the strength of bichrome test A, is prepared in like manner. ·7135 grain of bichromate of potash dissolved in 1000 grains of water will furnish a solution of such a strength; I call this solution the "bichrome test B." A "bichrome test C," one-tenth strength of solution B, is prepared by dissolving ·07135 grain of bichromate in 1000 grains of water. These solutions, A, B, and C, will contain chromic acid in 100 grains; equal in solution A to 1 grain of metallic bismuth, in solution B to 0·1 grain of bismuth, and in solution C chromic acid equal to ·01 grain of bismuth. On the correctness of these solutions of course depends the value of any results that may be obtained by their use.

In my own experiments I make use of a white glass flask capable of holding about 2000 grains of liquid. As it is necessary to keep the solution hot during the experiment, I use a clasp of iron plate with wooden handles; the flask having a rim, it can be slipt round the neck at pleasure.

It may be as well to notice the decomposition which takes place on the addition of bichromate of potash to a solution of

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bismuth. When bismuth is in solution as nitrate, it may be expressed thus:—

$$2BiO^3, NO^5 \times KO, 2CrO^3 = (2BiO^3\ CrO^3) \times KO, NO^5 \times NO^5.$$

The chromate of bismuth is of a rich yellow colour. In warm solutions it is deposited almost instantly. On the addition of one or two drops of bichromate only, the precipitate diffuses itself throughout the mixture, imparting a yellow milkiness to the whole fluid. Further addition of the bichrome test causes the aggregation of the precipitate; and on the experiment approaching completion, it coagulates speedily, leaving the supernatant liquor perfectly free from any floating particles, in this way affording facility for observing the effect of continued addition of the bichrome test. At the conclusion of the experiment, chromate of bismuth will cease to precipitate, and any further addition of bichrome test will remain in solution, and may be detected by its characteristic colour. An experiment such as just described will occupy about ten minutes.

The preceding remarks on the estimation of bismuth apply to colourless solutions only, and for such the method is specially adapted. In commercial analysis, however, it is desirable to analyse ores, alloys, &c. with great rapidity, approximate results only being required. A simple modification of the preceding method enables us to extend the volumetric mode of analysis to solutions of any degree of colour. A perfectly saturated solution of nitrate of bismuth (or a salt of lead), as near neutral as possible, is prepared, and small portions of the fluid dotted over a white porcelain slab. An alloy or other specimen in solution to be tested is treated in precisely the same manner as when testing a colourless liquid with a standard solution of bichromate of potash. The standard test-liquor, in analyses such as I now refer to, may be prepared of a much greater density than those previously described. When the experiment approaches completion, which may be readily ascertained even in coloured liquids by the precipitate produced, the fine point of a glass rod is inserted after each addition, and the moistened rod brought into contact with one of the dots of a solution of bismuth on the white slab. As soon as an excess of bichromate of potash remains in solution, on touching a dot of bismuth solution with the wet rod a film of chromate of bismuth will instantly form. So long as any bismuth remains in solution, the bismuth dots will remain perfectly clear; consequently the formation of a yellow milkiness in the bismuth dots replaces the colour of bichromate of potash itself as an indicator of the progress of the experiment.

The only source of error in the mass analyses originates in the circumstance of one or more drops of the bichrome test being

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required in excess to indicate the complete precipitation of the bismuth. The bichrome test necessary to effect this object will vary in amount, being greater as the volume of the liquid operated upon increases. The amount of bichromate in excess is calculated as being in combination with the bismuth, therefore the results obtained by this method are a little too high. The intensity of colour so peculiar to bichromate of potash reduces the required excess to an infinitely low figure, however. By reversing the course of procedure, *i. e.* adding a solution of bismuth to a solution of bichromate of potash, a contrary result naturally occurs. In the latter case we add a few drops of bismuth solution in excess to ensure the complete precipitation; consequently since the excess so added, as in the preceding case, is calculated as being combined with chromic acid, it follows that the quantity of bismuth deduced will be a little too low. We may get as accurate results as it is probably possible to obtain by combining the two methods in the following manner:—

Method I.—1000 grains of the solution in which bismuth is to be estimated are poured into a flask, heated to ebullition, and small portions at a time of a graduated solution of bichromate of potash added from a burette until a precipitate ceases to form and the supernatant liquid is coloured by an excess of the bichrome test. Suppose 250 grains of bichrome test B have been required to effect this object.

Method II.—To the ascertained volume of bichromate of pot ash, which we have supposed to be 250 grains, a graduated solution of nitrate of bismuth is added *vice versâ* until the colour of the liquid entirely disappears. The number of measures used is read off. Suppose 1025 grains of the solution of bismuth have been added, then the true quantity is as follows:—

There have been used for—

I.	1000 grs. of solution of bismuth,			250 grs. of bichrome test B.		
II.	1025	...	...	250	...	...
	2025			500		

Since 100 grains of bichrome test B represents 0·1 grain of bismuth, the 500 grains used indicate $\cdot 1 \times 5 = 0 \cdot 5$ grain of metallic bismuth as the quantity contained in 2025 grains of the solution of bismuth.

The following is an equally simple and sometimes more convenient modification:—

Method III.—As in the preceding cases, the bichrome test is added to a solution of bismuth of unknown strength until an excess of the precipitant remains in solution. A standard solution of bismuth is now added to the same solution from which the bismuth has been precipitated, until the colour of the super-

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natant liquid is entirely removed; by this means we may get the requisite data for correction.

The following experiments may serve as a critical illustration of the various methods described above:—

A. 5·640 grains of pure bismuth were dissolved in pure nitric acid, and the solution evaporated to a syrupy consistence; by evaporating in this way all excess of acid was expelled. So much limpid water was then added as to increase the volume to 2820 grs. The liquid was divided into two equal portions; one solution precipitated with bichromate of potash in excess, and the other with a mixture of caustic ammonia and carbonate of ammonia. In the latter case the mixture was allowed to stand at about 150° F. for eight hours. They contained respectively 2·820 grs. of metallic bismuth.

Amount indicated by the chromate of bismuth obtained . . .	2·8196 grs.
Amount indicated by the oxide of bismuth obtained	2·8182 grs.
Actual amount present	2·8200 grs. of bismuth.

B. From 1000 grains of a solution of nitrate of bismuth of unknown strength, the bismuth was precipitated by the insertion of a plate of zinc, and the metal calculated from the roasted oxide of bismuth. From precisely the same volume the bismuth was precipitated in combination with chromic acid by means of bichromate of potash. The amount of metal calculated from the use of—

Bichromate of potash =	2·174 grs.
Metallic zinc . . . =	2·112 grs.
Difference . . .	·062 gr. of bismuth.

C. A solution of nitrate of bismuth containing exactly 3·639 grs. of bismuth was mixed with 3·205 grs. of copper as sulphate, 1·240 gr. of manganese also as sulphate, and several grains each of nitrate of uranium, sulphate of zinc, and sulphate of iron.

Found from the chromate of bismuth	3·6386 grs.
Amount actually present	3·6390 grs.
Difference	·0004 gr. of bismuth.

D. To a solution containing 4·827 grains of bismuth, 5·100 grains of lead, and 3·724 grains of baryta, an excess of dilute sulphuric acid was added, the temperature slightly elevated, and the mixture filtered. After dissolving out the sulphate of bismuth with water slightly acidified with acetic acid, the filtrate was evaporated until fumes of sulphuric acid began to be emitted; the fluid was then diluted with water, and supersaturated with a

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solution of bichromate of potash, the precipitate warmed, filtered, and weighed. It contained:—

According to the chromate of bismuth obtained	4·8271 grs.
Amount actually present . . .	4·8270 grs.
Difference	·0001 gr. of bismuth.

E. A liquid which contained 4·983 grains of bismuth associated with 5·271 grains of lead was precipitated, and the resulting chromates of lead and bismuth then treated with caustic potash of spec. grav. 1·33. The liquid contained—

According to the chromate of bismuth obtained	4·9824 grs.
Amount actually present . . .	4·9830 grs.
Difference	·0006 gr. of bismuth.

The following experiments relate to the determination of bismuth by volume.

a. 6·544 grains of chemically pure bismuth were dissolved in the necessary quantity of pure nitric acid, and the solution evaporated to a pasty mass and diluted with distilled water to the mark which indicated 3272 grains liquid. Consequently 1000 grains contained 2 grains of metallic bismuth.

When tested according to method it was found to contain . .	2·0005 grs.
Amount actually present . . .	2·0000 grs.
Difference	·0005 gr. of bismuth.

b. 2·114 grains of bismuth were treated in a similar manner with pure nitric acid. To this solution several grains each of sulphate of zinc, nitrate of silver, nitrate of mercury, and sulphate of tin were added. The mixture was diluted to about 1000 grains.

Found according to Method I. .	2·1143 grs.
Found according to Method II. .	2·1136 grs.
Mean	2·1139 grs. of bismuth.

c. A liquid of an intense dark colour, containing 4·354 grains of bismuth mixed with varying quantities of copper, iron, molybdenum, and antimony, was treated in the usual way with a standard solution of bichromate of potash, the point of saturation being indicated by a bismuth solution, as previously described.

Found by Method *c* . . .	4·35 grs.
Amount actually present . .	4·24 grs.
Difference	0·11 gr. of bismuth.

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Quantitative Ratio of Bichromate of Potash and Metallic Bismuth.

In adopting centigrade methods of analysis, it becomes a matter of vital importance to determine with the greatest accuracy the atomic weight of the body to be estimated, and of the agent by which its determination is to be effected. The metal to be estimated by the method now described is bismuth. If we adopt with Gmelin the formula BiO^3 for the oxide of bismuth, we find that the following atomic weights have been assigned to the metal:—Berzelius, 213; Kane, 213·3; Graham, 213·21: Gmelin gives 210, or even less, as the atomic weight of bismuth. To the metal chromium, which of course materially affects that of bichromate of potash, the following atomic weights have been assigned:—26, 26·24, 27, and 28. The following Table gives the quantities of bichromate of potash that would be required to combine with 100 parts of bismuth, according to the various atomic weights that have been assigned to the metals chromium and bismuth. Potassium is taken at 39.

Atomic weights of bismuth.	Chromium at 26.	Chromium at 26·24.	Chromium at 27.	Chromium at 28.
210	70·00	70·22	70·95	71·90
213	69·01	69·24	69·84	70·88
213·21	68·88	69·17	69·78	70·82
213·3	68·10	69·14	69·75	70·79

Upon examining the above Table, it will be found that the maximum quantity of bichromate of potash required for 100 parts of bismuth is 71·90, and the minimum quantity 68·10, a difference too great to be attributed to experimental error when pure materials are employed. No correct data being deducible from the atomic weights given, it became necessary to ascertain by experiment the exact quantity of bichromate of potash required to combine with a known amount of bismuth. These data I obtained by dissolving a certain quantity of pure bismuth in nitric acid, and after removing excess of acid, I ascertained the quantities required to combine in two or three ways; first by addition of bichromate of potash to the solution of bismuth until an excess of bichromate gave rise to a yellow colour; after which I applied the method inversely, adding a solution of nitrate of bismuth of known strength to a given volume of bichromate of potash until the loss of colour indicated complete saturation of the solution. From the weight of the precipitated chromate of bismuth I also calculated the bichromate of potash that would be required.

The mean of all my experiments gave 71·35 parts of bichromate of potash as the quantity required to combine with 100

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parts of pure bismuth. This result may be taken, I feel satisfied, as the correct ratio upon which to base calculations in the estimation of bismuth. Upon this datum the standard solutions previously described are prepared.

In conclusion I may say, that the method I propose for the determination of bismuth recommends itself by the accuracy of its results, facility of execution, and neatness of manipulation, joined to the permanency of the test-agent employed.

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XXVII. *On the Dynamical Theory of Heat.*—Part VI. *Thermo-electric Currents.* *By* WILLIAM THOMSON, *M.A., Professor of Natural Philosophy in the University of Glasgow**.

[Continued from vol. ix. p. 531.]

Preliminary §§ 97–101. *Fundamental Principles of General Thermo-dynamics recapitulated.*

97. MECHANICAL action may be derived from heat, and heat may be generated by mechanical action, by means of forces either acting between contiguous parts of bodies, or due to electric excitation; but in no other way known, or even conceivable, in the present state of science. Hence thermo-dynamics falls naturally into two divisions, of which the subjects are respectively, *the relation of heat to the forces acting between contiguous parts of bodies*, and *the relation of heat to electrical agency*. The investigations of the conditions under which thermo-dynamic effects are produced, in operations of any fluid or fluids, whether gaseous or liquid, or passing from one state to the other, or to or from the solid state, and the establishment of universal relations between the physical properties of all substances in these different states, which have been given in Parts I.–V. of the present series of papers, belong to that first great division of thermo-dynamics—to be completed (as is intended for future communications to the Royal Society) by the extension of similar researches to the thermo-elastic properties of solids. The second division, or thermo-electricity, which may include many kinds of action as yet undiscovered, has hitherto been investigated only as far as regards the agency of heat in producing electrical effects in non-crystalline metals. In a mechanical theory of electric currents, communicated to the Royal Society, Dec. 15, 1851†, the application of the general

* From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read 1st May, 1854.

† See 'Proceedings' of that date, or Philosophical Magazine, 1852, where a sufficiently complete account of the investigations and principal results is given.

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laws of the dynamical theory of heat to this kind of agency was made, and certain universal relations precisely analogous to the thermo-elastic properties of fluids established in the previous treatment of the first division of the subject, were established between the thermo-electric properties of non-crystalline metals. The object of the present communication is to extend the theory to the phænomena of thermo-electricity in crystalline metals; but as recent experimental researches on air have pointed out an absolute thermometric scale*, the use of which in expressing the

* That is a scale defined without reference to effects experienced by any particular kind of matter. Such a scale, founded on general thermo-dynamic relations of heat and matter, and requiring reference to a particular thermometric substance only for defining the unit or degree, was, so far as I know, first proposed in a communication to the Cambridge Philosophical Society (Proceedings, May 1848, or Philosophical Magazine, October 1848). The particular thermometric assumption there suggested was, that a thermo-dynamic engine working to perfection, according to Carnot's criterion, would give the same work from the same quantity of heat, with its source and refrigerator differing by one degree of temperature in any part of the scale; the fixed points being taken the same as the 0° and 100° of the centigrade scale. A comparison of temperature, according to this assumption, with temperature by the air thermometer, effected by the only data at that time afforded by experiment, namely Regnault's observations on the pressure and latent heat of saturated steam at temperatures of from 0° to 230° of the air thermometer, showed, as the nature of the assumption required, very wide discrepances, even inconveniently wide between the fixed points of agreement. A more convenient assumption has since been pointed to by Mr. Joule's conjecture, that Carnot's function is equal to the mechanical equivalent of the thermal unit divided by the temperature by the air thermometer from its zero of expansion; an assumption which experiments on the thermal effects of air escaping through a porous plug, undertaken by him in conjunction with myself for the purpose of testing it (Philosophical Magazine, Oct. 1852), have shown to be not rigorously but very approximatively true. More extensive and accurate experiments have given us data for a closer test (Phil. Trans., June 1853), and in a joint communication by Mr. Joule and myself to the Royal Society of London, to be made during the present session, we propose that the numerical measure of temperature shall be not founded on the expansion of air at a particular pressure, but shall be simply the mechanical equivalent of the thermal unit divided by Carnot's function. We deduce from our experimental results, a comparison between *differences on the new scale from the temperature of freezing water*, and *temperatures centigrade of* Regnault's *standard air thermometer*, which shows no greater discrepance than a few hundredths of a degree, at temperatures between the freezing- and boiling-points, and, through a range of 300° above the freezing-point, so close an agreement that it may be considered as perfect for most practical purposes. The form of assumption given below in the text as the foundation of the new thermometric system, without explicit reference to Carnot's function, is equivalent to that just stated, inasmuch as the formula for the action of a perfect thermo-dynamic engine, investigated in § 25, expresses (§ 42) that the heat used is to the heat rejected in the proportion of the temperature of the source to the temperature of the refrigerator, if Carnot's function have the form there given as a conjecture, and now adopted as the definition of temperature.

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general laws of the dynamical theory of heat, both leads to a very concise mode of stating the principles, and shows the most convenient forms of the expressions brought forward in my former communication, the elementary theory of thermo-electricity in metals will be included in the investigations now communicated. I shall take the opportunity of introducing developments and illustrations, which, although communicated at the meeting of the Royal Society along with the original treatment of the subject, did not appear in the printed abstract; and I shall add some experimental conclusions which have since been arrived at, in answer to questions proposed in the former theoretical investigation.

98. Before entering on the treatment of the special subject, it is convenient to recal the fundamental laws of the dynamical theory of heat, and necessary to explain the thermometric assumption by which temperature is now to be measured.

The conditions under which heat and mechanical work are mutually convertible by means of any material system, subjected either to a continuous uniform action, or to a cycle of operations at the end of which the physical conditions of all its parts are the same as at the beginning, are subject to the following laws:—

Law I.—The material system must give out exactly as much energy as it takes in, either in heat or mechanical work.

Law II.—If every part of the action, and all its effects, be perfectly reversible, and if all the localities of the system by which heat is either emitted or taken in, be at one or other of two temperatures, the aggregate amount of heat taken in or emitted at the higher temperature, must exceed the amount emitted or taken in at the lower temperature, always in the same ratio when these temperatures are the same, whatever be the particular substance or arrangement of the material system, and whatever be the particular nature of the operations to which it is subject.

99. *Definition of temperature* and *general thermometric assumption.*—If two bodies be put in contact, and neither gives heat to the other, their temperatures are said to be the same; but if one gives heat to the other, its temperature is said to be higher.

The temperatures of two bodies are proportional to the quantities of heat respectively taken in and given out in localities at one temperature and at the other, respectively, by a material system subjected to a complete cycle of perfectly reversible thermo-dynamic operations, and not allowed to part with or take in heat at any other temperature: or, the absolute values of two temperatures are to one another in the proportion of the heat taken in to the heat rejected in a perfect thermo-dynamic engine

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working with a source and refrigerator at the higher and lower of the temperatures respectively.

100. *Convention for thermometric unit, and determination of absolute temperatures of fixed points in terms of it.*

Two fixed points of temperature being chosen according to Sir Isaac Newton's suggestion, by particular effects on a particular substance or substances, the difference of these temperatures is to be called unity, or any number of units or degrees as may be found convenient. The particular convention is, that the difference of temperatures between the freezing- and boiling-points of water under standard atmospheric pressure shall be called 100 degrees. The determination of the absolute temperatures of the fixed points is then to be effected by means of observations indicating the œconomy of a perfect thermodynamic engine, with the higher and the lower respectively as the temperatures of its source and refrigerator. The kind of observation best adapted for this object was originated by Mr. Joule, whose work in 1844* laid the foundation of the theory, and opened the experimental investigation; and it has been carried out by him, in conjunction with myself, within the last two years, in accordance with the plan proposed in Part IV.† of the present series. The best results, as regards this determination, which we have yet been able to obtain is, that the temperature of freezing water is 273·7 on the absolute scale; that of the boiling-point being consequently 373·7. Further details regarding the new thermometric system will be found in a joint communication to be made by Mr. Joule and myself to the Royal Society of London before the close of the present session.

101. A corollary from the second general law of the dynamical theory stated above in § 98, equivalent to the law itself in generality, is, that if a material system experience a continuous action, or a complete cycle of operations, of a perfectly reversible kind, the quantities of heat which it takes in at different temperatures are subject to a linear equation, of which the coefficients are the corresponding values of an absolute function of the temperature. The thermometric assumption which has been adopted is equivalent to assuming that this absolute function is the reciprocal of the temperature; and the equation consequently takes the form

$$\frac{H_t}{t} + \frac{H_{t'}}{t'} + \frac{H_{t''}}{t''} + \&c. = 0,$$

* "On the Changes of Temperature occasioned by the Rarefaction and Condensation of Air," see Proceedings of the Royal Society, June 1844; or, for the paper in full, Phil. Mag., May 1845.

† "On a Method of discovering experimentally the Relation between the Heat Produced and the Work Spent in the Compression of a Gas." Trans. R.S.E., April 1851; or Phil. Mag. vol. iv. p. 424.

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if t, t', &c. denote the temperatures of the different localities where there is either emission or absorption of heat, and $\pm H_t$, $\pm H_{t'}$, $\pm H_{t''}$, &c. the quantities of heat taken in or given out in those localities respectively. To prove this, conceive an engine emitting a quantity H_t of heat at the temperature t, and taking in the corresponding quantity $\frac{t'}{t}H_t$ at the temperature t'; then an engine emitting the quantity $\frac{t'}{t}H_t + H_{t'}$ at t', and taking in the corresponding quantity $t''\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}\right)$ at the temperature t''; another emitting $t''\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}\right) + H_t$ at t'', and taking in the corresponding quantity $t'''\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}+\frac{H_{t''}}{t''}\right)$ at t'''; and so on. Considering $n-2$ such engines as forming one system, we have a material system causing, by reversible operations, an emission of heat amounting to H_t at the temperature t, $H_{t'}$ at the temperature t', ... and $H_{t^{(n-2)}}$ at $t^{(n-2)}$; and taking in $t^{(n-1)}\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}+\ldots+\frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right)$ at the temperature $t'^{(n-1)}$. Now this system, along with the given one, constitutes a complex system which causes on the whole neither absorption nor emission of heat at the temperatures t, t', &c., or at any other temperatures than $t^{(n-1)}$, $t^{(n)}$; but gives rise to an absorption or emission equal to $\pm\left[t^{(n-1)}\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}+\ldots.+\frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right)+H_{t^{(n-1)}}\right]$ at $t^{(n-1)}$, and an emission or absorption equal to $\pm H_{t^{(n)}}$ at $t^{(n)}$. This complete system fulfils the criterion of reversibility, and, having only two temperatures at localities where heat is taken in or given out, is therefore subject to Law II.; that is, we must have

$$H_{t^{(n}} = -\frac{t^{(n)}}{t^{(n-1)}}\left[t^{(n-1)}\left(\frac{H_t}{t}+\frac{H_{t'}}{t'}+\ldots.+\frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right)+H_{t^{(n-1)}}\right]$$

which is the same as

$$\frac{H_t}{t}+\frac{H_{t'}}{t'}+\ldots.+\frac{H_{t^{(n-1)}}}{t^{(n-1)}}+\frac{H_{t^{(n)}}}{t^{(n)}}=0 \quad . \quad . \quad . \quad . \quad (1).$$

This equation may be considered as the mathematical expression of the second fundamental law of the dynamical theory of heat. The corresponding expression of the first law is

$$W+J\,(H_t+H_{t'}+\ldots.+H_{t^{(n-1)}}+H_{t^{(n)}})=0 \quad . \quad . \quad (2),$$

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where W denotes the aggregate amount of work spent in producing the operations, and J the mechanical equivalent of the thermal unit.

§§ 102–106. *Initial examination of Thermo-dynamic circumstances regarding Electric Currents in Linear Conductors.*

102. Peltier's admirable discovery, that an electric current in a metallic circuit of antimony and bismuth produces cold where it passes from bismuth to antimony, and heat where it passes from antimony to bismuth, shows how an evolution of mechanical effect, by means of thermo-electric currents, involves transference of heat from a body at a higher temperature to a body at a lower temperature, and how a reverse thermal effect may be produced, by thermo-electric means, from the expenditure of work. For if a galvanic engine be kept in motion doing work, by a thermo-electric battery of bismuth and antimony, the current by means of which this is effected passing, as it does, from bismuth to antimony through the hot junctions, and from antimony to bismuth through the cold junctions, must cause absorption of heat in each of the former, and evolution of heat in each of the latter; and to sustain the difference of temperature required for the excitation of the electromotive force, even were there no propagation of heat by conduction through the battery, it would be necessary continually, during the existence of the current, to supply heat from a source to the hot junctions, and to draw off heat from the cold junctions by a refrigerator :—Or, if work be spent to turn the engine faster than the rate at which its inductive reaction balances the electromotive force of the battery, there will be a reverse current sent through the circuit, producing absorption of heat at the cold junctions, and evolution of heat at the hot junctions, and consequently effecting the transference of some heat from the refrigerator to the source.

103. We see, then, that in Peltier's phænomenon we have a reversible thermal agency of exactly the kind supposed in the second law of the dynamical theory of heat. Before, however, we can apply either this or the first law, we must consider other thermal actions which are involved in the circumstances of a thermo-electric current; and with reference to the second law, we shall have to examine whether there are any such of an essentially irreversible kind.

104. It is to be remarked, in the first place, that a current cannot pass through a homogeneous conductor without generating heat in overcoming resistance. This effect, which we shall call the *frictional generation of heat,* has been discovered by Joule to be produced at a rate proportional to the square of the

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strength of the current; and, taking place equally with the current in one direction or in the contrary, is obviously of an irreversible kind. Any other thermal action that can take place must depend on the heterogeneousness of the circuit, and must be of a kind reversible with the current.

105. Now if in an unbroken circuit with an engine driven by a thermo-electric current, the strength of the current be infinitely small, compared with what it would be were the engine held at rest, or, which is the same, if the engine be kept at some such speed that its inductive electromotive force may fall short of, or may exceed, by only an infinitely small fraction of itself, the amount required to balance the thermal electromotive force of the battery, there will be only an infinitely small fraction of the work done by the current in the former case, or of the work done in turning the engine in the latter, wasted on the frictional generation of heat through the electric circuit. In these circumstances, it is clear, that whatever mechanical effect would be produced in any time by the engine from a direct current of a certain strength, an equal amount of work would have to be spent in forcing it to move faster and keeping up an equal reverse current for the same length of time; and as the direct and reverse currents would certainly produce equal and opposite thermal effects at the junctions, and elsewhere in all actions depending on heterogeneousness of the circuit, it appears that, were there no propagation of heat through the battery by ordinary conduction, Carnot's criterion of a perfect thermo-dynamic engine would be completely fulfilled, and a definite relation, the same as that which has been investigated (§ 25) already by considering expansive engines fulfilling the same criterion, would hold between the operative thermal agency and the mechanical effect produced. It appears extremely probable that this relation does actually subsist between the *part of the thermal agency which is reversed with the current* and the mechanical effect produced by the engine, and that the ordinary conduction of heat through the battery takes place independently of the electrical circumstances. The following proposition is therefore assumed as a fundamental hypothesis in the theory at present laid before the Royal Society.

106. *The electromotive forces produced by inequalities of temperature in a circuit of different metals, and the thermal effects of electric currents circulating in it, are subject to the laws which would follow from the general principles of the dynamical theory of heat if there were no conduction of heat from one part of the circuit to another.*

In adopting this hypothesis, it must be distinctly understood that it is only a hypothesis, and that, however probable it may

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appear, experimental evidence in the special phænomena of thermo-electricity is quite necessary to prove it. Not only are the conditions prescribed in the second law of the dynamical theory not completely fulfilled, but the part of the agency which does fulfil them is in all known circumstances of thermo-electric currents excessively small in proportion to agency inseparably accompanying it and essentially violating those conditions. Thus, if the current be of the full strength which the thermal electromotor alone can sustain against the resistance in its circuit, the whole mechanical energy of the thermo-electric action is at once spent in generating heat in the conductor,—an essentially irreversible process. The whole thermal agency immediately concerned in the current, even in this case when the current is at the strongest, is (from all we know of the magnitude of the thermo-electric force and absorptions and evolutions of heat) probably very small in comparison with the transference of heat from hot to cold by ordinary conduction through the metal of the circuit. It might be imagined, that by choosing, for the circuit, materials which are good conductors of electricity and bad conductors of heat, we might diminish indefinitely the effect of conduction in comparison with the thermal effects of the current; but unfortunately we have no such substance as a *non-conductor* of heat. The metals which are the worst conductors of heat are nearly in the same proportion the worst conductors of electricity; and all other substances appear to be comparatively very much worse conductors of electricity than of heat; stones, glass, dry wood, and so on, being, as compared with metals, nearly perfect non-conductors of electricity, and yet possessing very considerable conducting powers for heat. It is true we may, as has been shown above, diminish without limit the waste of energy by frictional generation of heat in the circuit, by using an engine to do work and react against the thermal electromotive force; but, as we have also seen, this can only be done by keeping the strength of the current very small compared with what it would be if allowed to waste all the energy of the electromotive force on the frictional generation of heat, and it therefore requires a very slow use of the thermo-electric action. At the same time it does not in any degree restrain the dissipation of energy by conduction, which is always going on, and which will therefore bear an even much greater proportion to the thermal agency electrically spent than in the case in which the latter was supposed to be unrestrained by the operation of the engine. By far the greater part of the heat taken in at all, then, in any thermo-electric arrangement is essentially dissipated, and there would be no violation of the great natural law expressed in Carnot's principle if the small part of the whole action, which is

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reversible, gave a different, even an enormously different, and either a greater or a less proportion of heat converted into work to heat taken in than that law requires in all completely reversible processes. Still the reversible part of the agency, in the thermo-electric circumstances we have supposed, is in itself so *perfect*, that it appears in the highest degree probable it may be found to fulfil independently the same conditions as the general law would impose on it if it took place unaccompanied by any other thermal or thermo-dynamic process.

§§ 107–111. *Mathematical expression of the Thermo-dynamic circumstances of Currents in Linear Conductors.*

107. In a heterogeneous metallic conductor, the whole heat developed in a given time will consist of a quantity generated *frictionally*, increased or diminished by the quantities produced or absorbed in the different parts by action depending on heterogeneousness of the circuit. The former, according to the law discovered by Joule, may be represented by a term $B\gamma^2$, in which B denotes a constant depending only on the resistance of the circuit. The latter, being reversible with the current, may be assumed, at least for infinitely feeble currents, to be in a given conductor proportional simply to the strength of the current; and hence the whole quantity of heat evolved in a given time must be expressible by a term of the form $-A\gamma$; where A, whether it varies with γ or not, has a finite, positive, or negative value when γ is infinitely small. Hence the whole heat developed in any portion of a heterogeneous metallic conductor in a unit of time must be expressible by the formula

$$-A\gamma + B\gamma^2,$$

where B is essentially positive; but A may be positive, negative, or zero, according to the nature of the different parts of the conducting arc. It may be assumed with great probability, that the quantities A and B are absolutely constant for a given conductor with its different parts at given constant temperatures; and that when the temperatures of the different parts of a conductor are kept as nearly constant as possible with currents of different strengths passing through it, the quantities A and B can only depend on γ, inasmuch as it may be impossible to prevent the interior parts of the conductor from varying in temperature, and so changing in their resistance to the conduction of electricity, or in their thermo-electric properties. In the present paper, accordingly, A and B are assumed to depend solely on the nature and thermal circumstances of the conductor, and to be independent of γ; but the investigations and conclusions would be applicable to cases of action with sufficiently feeble currents,

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probably to all currents due solely to the thermal electromotive force, even if A and B were in reality variable, provided the limiting values of these quantities for infinitely small values of γ be used.

108. Let us consider a conductor of any length and form, but of comparatively small transverse dimensions, composed of various metals at different temperatures, but having portions at its two extremities homogeneous and at the same temperature. These terminal portions will be denoted by E and E′, and will be called the *principal electrodes*, or *the electrodes of the principal conductor*; the conductor itself being called the *principal conductor* to distinguish it from others, either joining its extremities or otherwise circumstanced, which we may have to consider again.

Let an electromotive force be made to act continuously and uniformly between these electrodes; as may be done, for instance, by means of a metallic disc included in the circuit touched by electrodes at its centre and a point of its circumference, and made to rotate between the poles of a powerful magnet, an arrangement equivalent to the "engine" spoken of above. Let the amount of this electromotive force be denoted by P, to be regarded as positive, when it tends to produce a current from E through the principal conductor, to E′. Let the absolute strength of the current, which in these circumstances passes through the principal conductor, be denoted by γ, to be considered as positive if in the direction of P when positive.

109. Then $p\gamma$ will be the amount of work done by the electromotive force in the unit of time. As this work is spent wholly in keeping up a uniform electric current in the principal conductor, it must be equal to the mechanical equivalent of the heat generated, since no other effect is produced by the current. Hence if $-A\gamma+B\gamma^2$ be, in accordance with the preceding explanations, the expression for the heat developed in the conductor in the unit of time by the current γ, and if J, as formerly, denote the mechanical equivalent of the thermal unit, we have

$$P\gamma=J(-A\gamma+B\gamma^2) \quad . \quad . \quad . \quad . \quad . \quad (3),$$

which is the expression for the particular circumstances of the first fundamental law of the dynamical theory of heat.

Hence, by dividing by γ, we have

$$P=J(-A+B\gamma) \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

from which we deduce

$$\gamma=\frac{P+JA}{JB} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

110. These equations show, that, according as P is greater than, equal to, or less than $-JA$, the value of γ is positive, zero, or

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negative; and that, in any of the circumstances, the strength of the actual current is just the same as that of the current which an electromotive force equal to $P+JA$ would excite in a homogeneous metallic conductor having JB for the absolute numerical measure of its galvanic resistance. Hence we conclude:—

(1.) That in all cases in which the value of A is finite, there must be an intrinsic electromotive force in the principal conductor, which would itself produce a current if the electrodes E, E′ were put in contact with one another, and which must be balanced by an equal and opposite force, JA, applied either by means of a perfect non-conductor, or some electromotor, placed between E and E′, in order that there may be electrical equilibrium in the principal conductor.

And (2.), That JB, which cannot vanish in any case, is the absolute numerical measure of the galvanic resistance of the principal conductor itself.

It appears, therefore, that the whole theory of thermo-electric force in linear conductors is reduced to a knowledge of the circumstances on which the value of the coefficient A, in the expression $-A\gamma+B\gamma^2$ for the heat developed throughout any given conductor, depends.

110. To express the second general law, we must take into account the temperatures of the different localities of the circuit in which heat is evolved or absorbed, when the current is kept so feeble (by the action of the electromotive force P against the thermo-electric force of the system) as to render the frictional generation of heat insensible. Denoting then by $\alpha_t\gamma$ the heat absorbed in all parts of the circuit which are at the temperature t, by the action of a current of infinitely small strength γ: so that the term $-A\gamma$, expressing the whole heat generated not frictionally throughout the principal conductor in any case, will be the sum of all such terms with their signs changed, or

$$A\gamma=\Sigma\alpha_t\gamma,$$

which gives

$$\Sigma\alpha_t=A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6);$$

and denoting by F the value of the electromotive force required to balance the thermo-electric tendency; we have

$$F=J\Sigma\alpha_t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

The second general law, as expressed above in equation (1), applied to the present circumstances, gives immediately

$$\Sigma\frac{\alpha_t\gamma}{t}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8);$$

or, since γ is the same for all terms of the sum,

$$\Sigma\frac{\alpha_t}{t}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

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111. Of these equations, (7) and (3), from which (7) is derived, involve no hypothesis whatever, but merely express the application of a great natural law—discovered by Joule for every case of thermal action, whether chemical, electrical, or mechanical—to the electrical circumstances of a solid linear conductor having in any way the property of experiencing reverse thermal effects from infinitely feeble currents in the two directions through it. Equation (9) expresses the hypothetical application of the second general law discussed above in § 106. The two equations, (7) and (9), express all the information that can be derived from the general dynamical theory of heat, regarding the special thermal and electrical energies brought into action by inequalities of temperature, or by the independent excitation of a current in a solid linear conductor, whether crystalline or not. The condition that the circuit is to be linear, being merely one of convenience in the initial treatment of the subject, may of course be removed by supposing linear conductors to be put together so as to represent the circumstances of a solid conductor of electricity, with any distribution of electric currents whatever through it; and we may therefore regard these two equations as the fundamental equations of the mechanical theory of thermo-electric currents. To work out the theory for crystalline or non-crystalline conductors, it is necessary to consider all the conditions which determine the generation or absorption of heat in different parts of the circuit, whatever be the properties of the metals of which it is formed. This we may now proceed to do; first for non-crystalline, and after that for crystalline metals.

[To be continued.]

XXVIII. *Some Experiments showing the apparent Conversion of Electricity into Mechanical Force. By* W. R. Grove, *Esq., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a communication* made to the members of the Royal Institution, on Friday, January 25th, entitled "Inferences from the Negation of Perpetual Motion," I showed an experiment which, with one or two others now added, may be thought worth recording in the Philosophical Magazine. They are hardly, I think, likely to be deduced from our received electrical theories, though possibly not inconsistent with some of them.

* [An abstract of this communication will appear in our Number for April.—Eds.]

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My object was to show, that when electricity performs any mechanical work which does not return to its source, electrical power is lost. The first experiment was made in the following manner:—A Leyden jar of one square foot coated surface has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about $\frac{1}{2}$ an inch apart). Between the Leyden jar and the prime conductor is inserted a small unit-jar of 9 square inches surface, the knobs of which are 0·2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between the attracting knobs, and the Leyden jar charged by discharges from the unit-jar. After a certain number of these (twenty-two in the experiment performed in the theatre of the Institution), the discharge of the large jar takes place across the $\frac{1}{2}$-inch interval; this may be viewed as the expression of electrical power received from the unit-jar. The experiment is now repeated, the wire between the balls having been removed, and therefore the 'tip' or the raising of the weight is performed by the electrical repulsion and attraction of the two pairs of balls; at twenty-two discharges of the unit-jar the balance is subverted, and one knob drops upon the other, but *no discharge takes place*, showing that some electricity has been lost, or converted into the mechanical power which raises the balance. By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and would be restored if the ball were brought back, without discharge, by extraneous force.

The experiment is believed to be new, and to be suggestive of others of a similar character, which may be indefinitely varied. Thus, two balls made to diverge by electricity should not give to an electrometer the same amount of electricity as if they were, whilst electrified, kept forcibly together: an experiment of this sort I have made since my lecture in the following manner. To a thick brass wire, 2 feet long, insulated and terminated by knobs, are suspended by fine platina wires, two pairs of discs of paper coated with tinfoil, and 4 inches in diameter. The apparatus is electrized in a dry atmosphere by sparks from a machine, and the discs of each pair respectively diverge. To one of the pairs a silk thread is attached, by which the discs can be forcibly approximated, and as often as this is done, the divergence of the other pair increases.

Another mode of showing the same effect is the following. On the top of an ordinary gold-leaf electroscope place two brass plates, such as those commonly used for a condenser, connect them by a long fine wire, and electrify them by a rubbed rod of

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glass or sealing-wax, so that the gold leaves diverge. Now raise the upper plate by a glass handle: the leaves collapse in proportion as it is raised and again diverge as it is depressed. It should be recollected that the plates are electrified by the same electricity, and are always metallically connected by the fine wire, in which respect this differs from ordinary induction experiments. It may be said that here the mechanical force is given by the hand; but this is only in part, the repellent effect of electricity does part of the work and would be therefore expended; it is analogically as though a man were to add his force to the piston-rod of a steam-engine, which would not prevent the loss of heat by the dilating steam. I had hoped to have carried the experiments further and examined the relative quantities, but unfortunately I have no time for such inquiry, and must leave it to others more happily circumstanced.

Yours, &c.,

W. R. Grove.

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 163.]

June 21, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"Experimental Researches on the Movement of Atmospheric Air in Tubes." By W. D. Chowne, M.D.

In the year 1847, the author of this paper made numerous experiments for the purpose of ascertaining what are the conditions under which atmospheric air is placed with regard to motion or rest, when within a vertical tube having one extremity communicating within the interior of a building, and the other in the open atmosphere.

The paper now submitted to the Royal Society contains the results of investigations undertaken in the year 1853 and continued to the present time, to ascertain whether the ordinary state of atmospheric air contained in a vertical cylindrical tube, open at both ends, and placed in the still atmosphere of a closed room, is one of rest or of motion; and if of motion, to investigate the influences of certain changes in the condition of the atmosphere which either produce, promote, retard, or arrest the movement.

He demonstrates, by a series of experiments, that when a tube, open at both ends, is placed in a vertical position, every precaution being taken to exclude all extraneous causes of movement in the surrounding atmosphere, an upward current of air is almost immediately established, and continued so long as these conditions are maintained.

The experiments were made in a room 12 feet square by 8 feet 6 inches high; the window and chimney being carefully secured, and

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all crevices closed, by pasting paper over them, the floor carpeted, the door double, and the inner door surrounded with list. The outer wall, having a north aspect, was so sheltered by surrounding buildings that the direct rays of the sun never fell upon the window. Discs of delicate tissue-paper were suspended in several parts of the room, to indicate currents of air, if any existed, and observations were taken only when these were perfectly quiescent.

Mason's hygrometer was first employed in these experiments, to test the presence of a current of air in the tube; on the principle that as evaporation produces cold, and as evaporation is increased by a current of air, the wet-bulb thermometer would show a greater depression if any current existed, than if the air were perfectly quiescent within the tube. The tube (fig. 1) was placed in the middle of the room, and isolated from the floor by a cylinder of thick glass laid under it.

It was found that in ninety-one observations of the hygrometer, suspended in the free air of the room, the mean depression of the wet-bulb thermometer was 3°·9 Fahr., while in ninety corresponding observations, with the hygrometer at the lower aperture, O, of the tube, the mean depression was increased to 4°·9 Fahr., clearly indicating the existence of a current of air within the tube.

Partial closure of the upper orifice of the tube, by placing a piece of fine muslin upon it, produced a sensible influence on the hygrometer. In seventeen observations with the tube thus partially obstructed, the mean depression was 2°·5 Fahr.; but in an equal number of comparative observations, with the tube perfectly free, the mean depression was increased to 3°·12 Fahr.; showing a considerable diminution of the force of the current within the tube, as a result of the partial obstruction of its upper aperture.

Fig. 1. Fig. 2. Fig. 3.

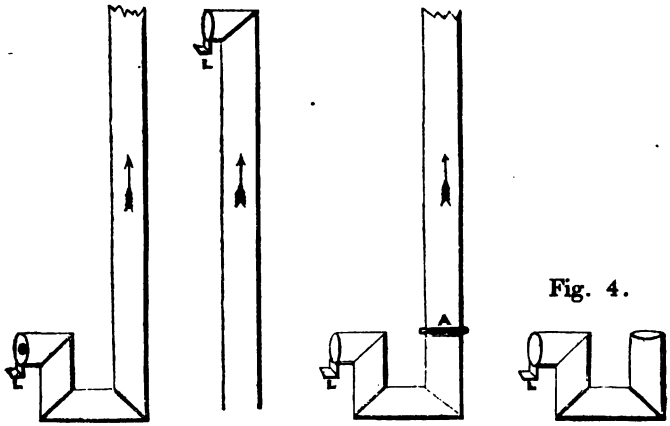

Fig. 4.

L. A ledge or step to place the hygrometer upon.
O. Orifice against which the bulbs were placed.

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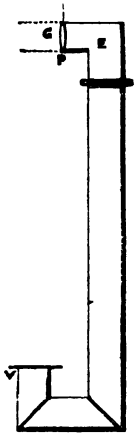
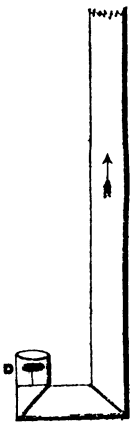
Similar comparative observations, with the hygrometer placed at the upper aperture of the tube (fig. 2), yielded similar results.

In these experiments the lower extremity of the vertical tube was bent thrice at right angles*, for convenience in making the observations, and it appeared desirable to ascertain what influence the long branch of the siphon-like tube had in the production of the current. For this purpose the long vertical tube (fig. 3) was made moveable at A, so that the apparatus could be alternately converted into a siphon with equal limbs 4 inches in length (fig. 4), or one with a short leg of 4 inches, and a long one of 96 inches (fig. 3). In twelve observations, when the long leg was inserted, the mean depression of the hygrometer was 2°·5 Fahr.; when the limbs were of equal length, 2°·25 Fahr.

Considering it possible that the current of air existing in the tube might have sufficient force to move a light body delicately suspended in its track, an elbow, E (fig. 5), was inserted into the upper orifice of the tube, to which a piece of glass tube, G, of the same diameter, was adapted, 6 inches in length, and a disc of tissue-paper, weighing one grain, which nearly occupied the area of the tube, was delicately suspended by a hair, at right angles to the axis of the tube. A slide valve was so adapted to the lower orifice, that this aperture could be opened or closed without entering the room. The air of the room being quiescent, it was found that when the slide valve closed the lower orifice of the tube, the disc of tissue paper remained perfectly quiescent; but that when the slide valve was withdrawn, leaving the lower orifice open, oscillations of the paper occurred, and it was projected

Fig. 5. Fig. 6.





V. Slide valve. D. A disc which occupies nearly the whole area of the tube.

* The tubes used in these experiments were bent either at their lower or upper extremities for convenience merely.

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at a small angle towards the upper orifice of the tube, demonstrating the existence of a feeble current of air through the apparatus.

The preceding experiment having proved the existence of a current of air within the tube, of sufficient force to move a light body, the author next proceeded to ascertain the velocity of the current by means of an anemometer, in the form of a horizontal fly-disc, D, suspended within the lower orifice of a tube (fig. 6), bent twice at right angles below. The revolving disc was made of a circular piece of stout writing-paper, cut into twenty-four equal segments, from the circumference to near the centre, each of the segments being afterwards inclined at an angle of twenty-five degrees*, like the vanes of a windmill; so that when properly suspended, a current of air entering the lower orifice of the tube would cause the disc to revolve from right to left. The disc was suspended in the same manner as the needle of the mariner's compass, and by the same means.

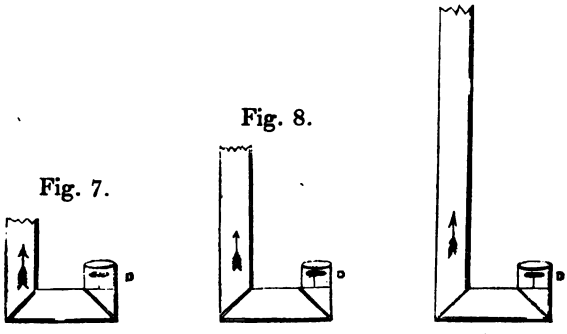
When the apparatus was arranged, the door of the room closed, and the atmosphere in a quiescent state, it was found that a constant regular rotation of the disc was established, and kept up by the upward current of air through the apparatus, and continued so long as the atmosphere of the room was quiet; but that agitations of the surrounding air either rendered the rotation uncertain, or reversed it.

Having thus ascertained that the current of air within the vertical tube possessed sufficient force to cause the rotation of a lightly suspended fly-disc, the question arose, what influence elongation or

Fig. 9.

Fig. 8.

Fig. 7.



shortening of the tube would exert on the velocity of the current. For this purpose three tubes (figs. 7, 8, 9) of precisely similar construction, but with long limbs of 12, 24, and 48 inches respectively, were fitted as before with fly-discs, D, D, D, and placed near each other in the centre of the room.

In nineteen observations, the number of revolutions in the tube, with a long limb of 12 inches, varied from 0·75 to 4·5 per minute;

* A nearer approach to an angle of 45 would have crippled the paper, so that it would not have preserved the horizontal position.

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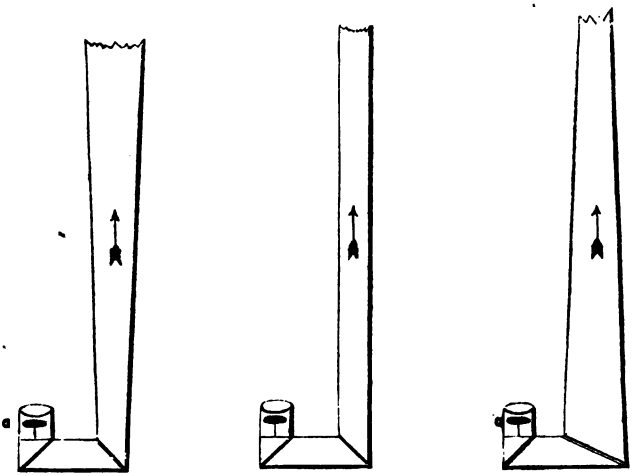
in that with a long limb of 24 inches, from 1·5 to 9·0, and in that with the long limb of 48 inches, from 3·75 to 14·0 per minute. The gross number of revolutions in the three tubes, in the nineteen observations, were respectively 51·25, 111·25, and 199·75; and the mean revolutions per minute, 2·697, 5·855, 10·513, which, allowing for errors of observation, yield the ratios 1, 2, 4 nearly; so that it may be said that the velocity of the revolutions is in a direct ratio to the lengths of the vertical tubes.

The influence of the conoidal form of the tube being suggested by Dr. Roget as worthy of investigation, a tube (fig. 10), 96 inches long by 3 inches diameter below and 6 inches above, was fitted to a rectangular tube containing the rotating disc D. Another tube (fig. 11) of the same length, 3 inches in diameter throughout, was

Fig. 10. Fig. 11. Fig. 12.



placed near the conical tube as a term of comparison. The revolutions of the disc in the conical tube were more rapid than in that of uniform diameter, in the proportion of 8·8 to 3·0. When the position of the cone was reversed (fig. 12), and the entrance and exit orifices were equal, the revolutions still continued more rapid than in the tube of uniform diameter.

To determine the influence of the area of the tube on the velocity of the current, four tubes (figs. 13, 14, 15, 16), 96 inches in length in the long, and 4 inches in the short branch, but varying in diameter, were placed in the room near each other and simultaneously observed.

In a tube of 3 inches uniform diameter (fig. 13), the revolutions were 3·0 per minute; in one of 5 inches (fig. 15) 9·15, and in one of 6·75 inches (fig. 16) 13·15; their respective areas being 7·065, 15·708, and 21·205. In the conical tube (fig. 14) on its base, whose area was 14·529, the revolutions were 8·8 per minute. It

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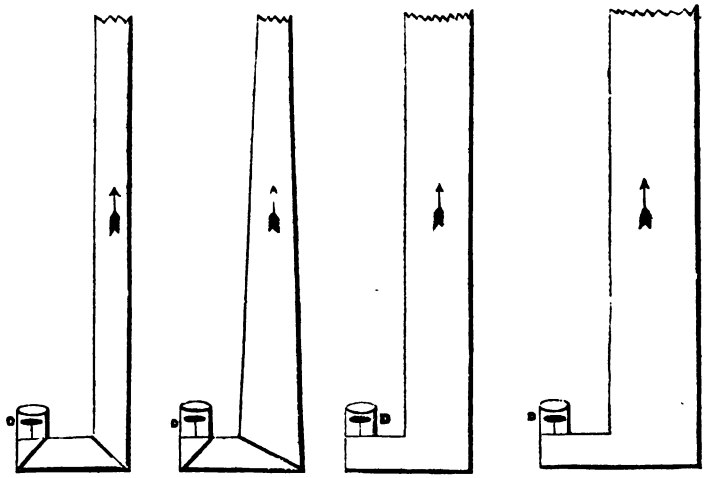
would seem, then, that the velocity has relation rather to the mean area of the tube than to that of the entrance and exit orifices, as the latter were the same in the tube of 3 inches uniform diameter, and in the conical tube on its base, while the revolutions of the disc were 3·0 per minute in the former, and 8·8 per minute in the latter.

Fig. 13. Fig. 14. Fig. 15. Fig. 16.



When the exit orifice of the tube of 6·75 inches diameter was reduced to 3·5 inches, the rapidity of the revolutions was reduced only about 10 per cent.

The influence of temperature in accelerating or retarding the currents through the tubes next engaged the author's attention; but before entering into direct experiments, he found by very numerous observations, that on some occasions no appreciable difference could be observed in the temperature of the atmosphere of the room near the floor and the ceiling, while on others there was a mean excess of 0°·17 Fahr. near the ceiling without causing any perceptible difference in the velocity of the revolutions of the discs. In forty comparative observations of the temperature of the external surface of the tube (fig. 13) and of the surrounding air, that of the tube was 0·09 higher; in twenty-three it was equal, but in only five was it lower than the surrounding air.

Of thirty-six comparative observations of the temperature of the air within, and external to the tube, by a delicate mercurial thermometer, it was found to be slightly higher within the tube in twenty-seven, and in the remaining nine it was equal, but never lower than that of the external air. The greatest excess was 0°·4 Fahr., and the mean excess 0°·14 Fahr.

The accuracy of these results was tested by an extremely delicate differential thermometer, which also indicated a minute excess of

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temperature within the tube; the author is led however to infer, that in the thirty-six observations the mean of 0°·14 is rather above the true excess; taking this however to be the exact amount, and as the atmospheric air is increased only $\frac{1}{480}$ of its volume, for every degree of Fahrenheit's thermometer, we shall have $\frac{14}{100}$ of $\frac{1}{480}$ for the increase of the whole bulk of that in the tube.

The disc continued to rotate while the thermometer indicated equal temperature in the tube and external to it, in eight of the cases, and was arrested by an accident in the ninth.

In another experiment, when the lower orifice of the tube was alternately closed and opened by a valve, the temperature appeared under both circumstances to be the same; hence, if we assume that a minute excess of temperature of the air within the tube, over that of the air external to it, exists, yet the experiment shows that it is not attributable to any heat being disengaged by the movement of the air itself.

Increase and decrease of the temperature of the room exercised a considerable influence on the velocity of the rotations of the discs, which increased as the day advanced, and declined as the temperature fell towards evening, although the direct rays of the sun never fell upon the window of the room.

Partial exclusion of light, by a blind covering the whole window, produced a considerable reduction in the velocity of the rotations of the discs, but a screen of a foot in breadth, interposed between the window and an individual tube (fig. 13), merely reduced the velocity of the rotations from 12·5 to 11·0 per minute.

The influence of reduction of temperature of the long branch of the tube, by placing around it two coils of wet tape*, reduced the revolutions of the disc from 4·0 to 1·75 per minute; a third reduced the revolutions to 1·0; a fourth to 0·5; and a fifth caused complete cessation.

To ascertain the influence of the abstraction of aqueous vapour on the rotation of the discs, a shallow vessel, containing strong sulphuric acid, was placed, at the suggestion of the Rev. Dr. Booth, immediately below the disc (D), in the short branch of the tube (fig. 17). After the lapse of thirty minutes, the rotation had ceased altogether; at the commencement the disc was rotating at the usual rate. The same vessel, placed in the tube without the sulphuric acid, had no effect on the rotation.

In another experiment a bell-glass was suspended over the short branch of the tube (fig. 18), so that the short branch projected into it, and a saucer (*s*), containing concentrated sulphuric acid, was also placed under the bell-glass, on a level with the orifice of the tube. The rotations of the disc were accelerated by placing the warm hand for a few seconds in contact with the long branch of the tube; but at the end of five minutes after it was withdrawn, and the room left and closed, the disc had ceased to rotate.

To determine the influence of partial abstraction of aqueous vapour from the entire atmosphere of the room on the velocity of

* Half an inch broad, and not so wet that any of the water ran away from it.

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the rotations, the three tubes (figs. 7, 8, 9), with long limbs of 12, 24,

Fig. 17.

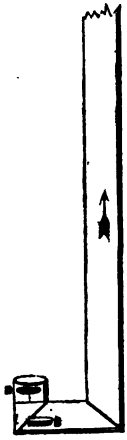
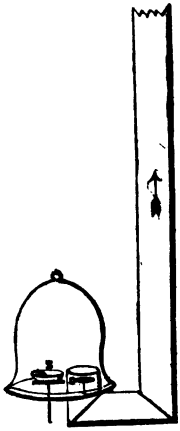


Fig. 18.



and 48 inches, employed in a previous experiment, were placed near each other, and three bushels of quicklime were spread in shallow vessels on the floor and other parts of the room. Before the lime was placed, the disc in the 12-inch tube was revolving at the rate of 0·75 per minute; that in the 24-inch tube at 2·0; and that in the 48-inch tube at 4·0 per minute. At the end of fifty minutes the rotation had ceased in the 12-inch tube, and was reduced to 1·75, and 3·5 in the 24- and 48-inch tubes. After seventy minutes, rotation had ceased in the 24-inch tube, and was reduced to 3·75 in the 48-inch tube. Finally, after ninety minutes, the rotations in the 48-inch tube were reduced to 2·75 per minute.

Similar reductions in velocity were observed after the removal and reintroduction of the quicklime in a second and third series of observations. Thus in all these experiments the rotations in the 12- and 24-inch tubes entirely ceased; and those in the 48-inch tube, although continued, were much diminished; a result most probably attributable to the greater quantity of aqueous vapour remaining in the upper strata of the air in the room.

The mean depression of the wet-bulb thermometer, the hygrometer being placed 48 inches above the floor, and the lime being absent, was 3·2; when the lime was present, 3·4. When the hygrometer was on the floor, the depression of the wet bulb was 3·5.

As the abstraction of aqueous vapour from the atmosphere diminished and even abolished the currents of air within the tubes, it was to be expected that increase of vapour in the atmosphere would produce the contrary effect, and accelerate the currents and the corresponding revolutions of the discs, and the following results coincide with that expectation.

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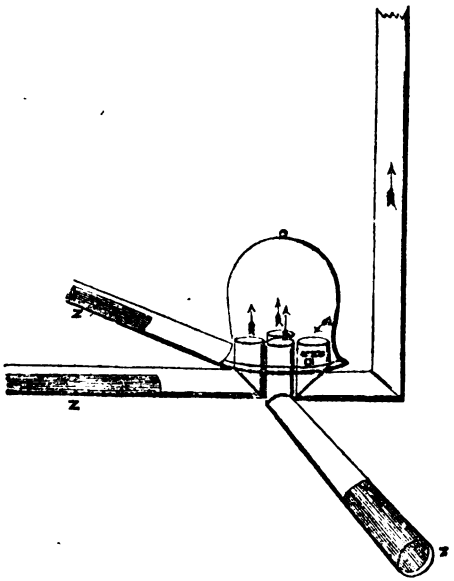
In the first experiment, the tube and bell-glass previously described (fig. 18) were employed, but substituting folds of damp linen for the saucer of sulphuric acid, so as fully to charge the air in the bell-glass with vapour, the rotations rapidly rose from 4·0 to 17 or 18 per minute.

But as the cold produced by the evaporation of the water in this experiment might be a source of fallacy, an arrangement was made (fig. 19) to supply the bell-glass with air, previously charged with vapour, formed at a distance of 5 feet from the glass. The rapidity of the revolutions was however still considerably increased.

Augmentation of the quantity of aqueous vapour, in the general atmosphere of the room, by spreading wet cloths on the floor and other parts, also produced increase in the rapidity of the rotations, though to a small extent.

Fig. 19.



Z, Z, Z. Tubes containing wet linen within their remote extremities.

These experiments* would seem to demonstrate that the ordinary condition of atmospheric air within vertical tubes, open at both extremities, is one of continual upward movement.

If the atmosphere were a strictly homogeneous elastic fluid, and

* Throughout the entire series the results were carefully observed during the night, when the atmosphere of the room was free from solar influences. The dry- and the wet-bulb thermometers yielded the same relative differences, and the discs rotated with the same constancy. The night as well as the day observations were continued through all the changes of temperature, from March 1853 to the present time.

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in a state of perfect equilibrium, any portion of it contained in a vertical tube would of course be perfectly stationary unless some adventitious cause produced disturbance of its equilibrium. But our atmosphere being a mixed fluid, and the aqueous vapour being of a much lower specific gravity at all atmospheric temperatures than the compound of which it forms a part, it is constantly rising within a tube, as in the free air; entering at the lower, and making its exit at the upper orifice of the tube.

The experiments appear further to demonstrate, that the presence of aqueous vapour in the atmosphere is essential to the production of the current within the vertical tubes, since by the abstraction of vapour from the air by quicklime, the rotations of the discs were invariably either diminished or caused to cease; while on the other hand, when the proportion of aqueous vapour in the air was increased, the currents and the rotations of the discs were simultaneously accelerated.

In concluding the details of these experiments, the author considers that they all tend to prove the existence of an upward current, under the circumstances described in the commencement of this paper.

They moreover yield a series of results which he hopes the Society will deem to be not without interest.

These results show it to be probable, if not certain, that the ordinary temperature of air within tubes, under the circumstances in which these were placed, is higher than of that external to them, all other relations of the tubes and surrounding objects being the same; they also show, that in eight instances, when the thermometers indicated an equality of temperature, within and external to the tube, the rotations of the discs still continued; and that when four coils of tape, moistened with water, were applied round the external surface of the tube, the rotations of the disc did not wholly cease.

They also show, that when the atmosphere of the room, in which the tubes were immersed, contained a larger or smaller proportion of aqueous vapour, all other things being equal, the discs revolved with more or less velocity; but that when the atmosphere was deprived in a great degree of aqueous vapour by the presence of quicklime, the thermometric state in all other respects remaining the same, the revolutions of the discs ceased.

Adverting to the indications cited above, of a minute excess of temperature in the interior of the tubes, and assuming that even that slight excess would be sufficient to rotate the discs, still the rotations diminished or ceased in proportion as the aqueous vapour was withdrawn.

Any increase of temperature which might have been produced by the quicklime would have had a tendency rather to increase than diminish the revolutions of the discs, but we have seen that the abstraction of the vapour entirely arrested their rotation.

With regard to the specific influence of each of the circumstances and agents most probably concerned in producing the phenomena described above, such as protection of the air within the tube from lateral expansion and mechanical agitations, to which the external

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air is exposed; gaseous diffusion; the unequal specific gravity of air and vapour; and the subtle operations of temperature at all times, the author is fully conscious that he has not ascertained their respective values.

He is also conscious that the phenomena themselves are the chief ground on which he can rest a claim for originality, and that the explanation of them may be better treated by those who are more accustomed to deal with similar researches.

In the course of these experiments the author has been especially indebted for many valuable suggestions to the Rev. Dr. Booth, Dr. Roget, Professor Sharpey, and Mr. Bishop; and he is also under obligations to Professor Stokes and to Mr. Brooke.

GEOLOGICAL SOCIETY.

December 19, 1855.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Description of a fossil cranium of the Musk-buffalo (*Bubalus moschatus*, Owen; *Bos Pallasii*, DeKay; *Ovibos Pallasii*, H. Smith and Bl.), from the Gravel at Maidenhead, Berks." By Prof. Owen, F.G.S.

This specimen was discovered by the Rev. Mr. Kingsley and Mr. J. Lubbock in a gravel-pit close to the engine-house at the Maidenhead station last summer, and is the first example of the subgenus *Bubalus* yet recognized as fossil in Britain. It consists of the cranial part of the skull, with the horn-cores, nearly perfect. The Professor, in describing this fossil, first offered his reasons for regarding the so-called "Musk-ox" as having been unnecessarily separated from the Buffaloes, and then gave an account of the few fossil skulls of the Musk-buffalo yet known, viz. those figured by Pallas, Ozeretskowsky, and Cuvier. A comparison was then made of the fossil remains with recent crania; and, although the skulls somewhat differ in a few points, especially in the relative curvatures of the horn-cores, yet the author was led to conclude that, as far as the materials for comparison at his command would serve, the differences between the fossil and recent Musk-buffaloes are not of specific value; that the *Bubalus moschatus* of the Arctic regions, with its now restricted range, is the slightly modified descendant of the old companion of the Mammoth and the Tichorine Rhinoceros, which with them enjoyed a much wider range, both in latitude and longitude, over lands that now form three divisions or continents of the northern hemisphere; and that the circumstances which have brought about the probably gradual extinction of the northern Rhinoceros and Elephant have not yet effected that of the contemporary species of arctic Buffalo.

2. "Note on the Gravel near Maidenhead, in which the remains of the Musk-buffalo were found." By Joseph Prestwich, Esq., Sec. G.S.

From Maidenhead to the sea, a distance of 50 miles, the valley of

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the Thames is occupied with a mass of ochreous gravel, from 5 to 15 feet thick, and varying from 2 or 3 miles to 8 or 9 miles in width. This gravel is composed of subangular chalk-flints, derived from the chalk of the adjacent district, together with flint-pebbles derived from the tertiary strata, and pebbles of quartz and old rocks derived from the conglomerates of the New Red Sandstone. There are also a few fragments of Oolitic rocks and of the Lower Greensand. Land-shells and bones of land-animals have been found in this gravel at several detached localities, as at Brentford, Kingston, London, &c. The date of the deposition of the mammaliferous gravel is, in the author's opinion, probably posterior to that of the boulder-clay of Norfolk and Suffolk, and necessarily posterior to the gravel which caps the chalk plateau traversed by the valley at Maidenhead. This latter, or "high-level," gravel is very similar in its lithological character to that in the valley, or the "low-level," gravel. The "low-level" gravel at Maidenhead rests on chalk-rubble; and the skull of the Musk-buffalo was found, together with fragments of other bones, low down in the gravel, where it begins to be mingled with the chalk-rubble.

3. "On some Geological Features of the country between the South Downs and the Sussex Coast." By P. J. Martin, Esq., F.G.S.

In this paper the author appropriates the boulder-drift lately brought to light by Mr. Godwin Austen to an outer zone of Wealden drift, in addition to those which he has already described as mantling round the nucleus of the Weald; the corresponding parts of this zone he thinks are to be found in the valley of the Thames, and perhaps yet to be discovered amongst the Greywethers and other relics of the Tertiaries found on the chalk country of Hampshire and Wilts. The above-mentioned zone the author considers as the remains of the boulder deposit spread over the tertiary countries of this and the adjoining parts of the North of Europe, before their continuity was disturbed by the upheaval of the great anticlinal of the South of England.

The country immediately under review, Mr. Martin regards as a sectional part of this great anticlinal, and not to be considered apart from the wide geological area to which it belongs.

He considers that its phænomena of arrangement and drift belong to the epoch of that upheaval, and betoken the agencies of powerful diluvial currents, set in motion and contemporaneously assisted by the dislocations known to abound in this part of our island; and without the aid of which no satisfactory conclusion, in the author's opinion, can be deduced respecting the drifts and the other phænomena of the denudations and surface-changes here exhibited.

Feb. 6, 1856.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Notice of the Raised Beaches in Argyllshire." By Commander J. E. Bedford, R.N. Communicated by Sir R. I. Murchison, V.P.G.S.

In June last some notes by Capt. Bedford were read on this sub-

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ject, in which he described two examples of raised beaches in the Lunga Islands and one in Kerera, all having an altitude of 40 ft. 8 in. above high-water mark. Other raised beaches were noticed,—one in Oronsay, at 38 ft. 6 in., and three in Jura, at 34 ft. 8 in., 42 ft. 1 in., and 105 ft. 5 in., respectively. During the last autumn Capt. Bedford kindly supplied some further notes and another highly finished map, comprising the raised beaches of Jura. He observed that these old beaches of the Western Isles were remarkable for their uniformity of level, their uniform horizontality, their vast extent of shingle, varying from highly polished pebbles to great rough blocks, and for their perfect state of preservation.

2. "On the Section exposed in the Excavation of the Swansea Docks." By M. Moggridge, Esq. Communicated by Sir R. I. Murchison, V.P.G.S.

The author of this paper gave a short account of the alternate beds of peat and marine clay exhibited in 1854 in the digging of the docks at Swansea. The best section presented the following series (in descending order):—1. Made ground, sand, and loose gravel, of variable thickness (from 20 to 6 feet): 2. Peat, with leaves, 2 feet: 3. Blue clay with *Scrobicularia piperata*, 8 ft. 6 in.: 4. Peat, of rather greater density than No. 2, 10 in.: 5. Blue clay with *Scrobicularia*, 4 ft. 1 in.: 6. Peat, with trees, 3 ft. 1 in.: 7. Brown clay and gravel, not penetrated. The valves of the *Scrobicularia piperata* occurred in pairs throughout the blue clays, but chiefly in their upper portions; it still lives on the coast, and burrows in similar clay in the estuaries. The plants forming the peat have in many instances left their roots in the underlying blue clays, proving that the peat was formed of terrestrial plants living and dying where they now are, and not of accumulated sea-weed or drifted material. Hence the section exhibits an interesting case of the frequent alternation of terrestrial and marine deposits over the same area.

3. "Notice of the recent Eruption of Manna Loa, in Hawaii." By W. Miller, Esq., H.M. Consul-General for the Sandwich Islands. From the Foreign Office.

The late volcanic eruption in the Sandwich Islands broke out in August last near the summit of Mauna Loa, which is 14,000 feet high and sixty miles from Hilo, Byron's Bay, in Hawaii. The stream of lava, having a breadth of from two to three miles, continued to flow in a north-east direction until the end of October, when the lava current, after having traversed a great part of the dense forest, appeared to have been checked in its progress at about three or four miles from the town of Hilo.

4. "Experimental Researches on the Granites of Ireland." By the Rev. Prof. S. Haughton, A.M., F.G.S.

The first part of this paper described the granites of the south-east of Ireland, which are reducible to three types, depending on their chemical and mineralogical composition. The granite of the first type, which Mr. Haughton proposed to call "Potash-granite," is found in the main granitic chain of Wicklow and Wexford, and

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also at Carnsore, at the extreme south-east of Ireland. The granite of the second type, which is a "Soda-granite," occurs at Rathdrum and Oulart, and is distinguished from the former by a diminution of silica and an increase of lime and soda. The third granite is peculiar, and found only at Croghan Kinshela, near the gold-mines of Wicklow. It consists of quartz, albite, and chlorite; while the potash-granites of the main chain consist of quartz, orthoclase, and margarodite mica.

The second part of the paper described the three granitic districts of the north-east of Ireland, known as the Mourne, Carlingford, and Newry granitic districts.

The granite of the Mourne district consists of quartz, orthoclase, albite, and a green mica, probably similar to margarodite.

The Carlingford granite is a potash-granite, in which hornblende replaces mica. At the junction of this granite with the Carboniferous Limestone of the neighbourhood, a remarkable change takes place in the granite on penetrating the limestone in dykes. From being originally a compound of quartz, orthoclase, and hornblende, it is converted by the addition of lime into a compound of hornblende and anorthite, which latter mineral was noticed for the first time as entering into the composition of British rocks.

The Newry granites belong to the "soda-granite" type, and resemble in many respects the secondary granites of the Wicklow and Wexford districts.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. viii. p. 236.]

Nov. 26, 1855.—A paper was read on the Earthquake in Switzerland in July last, by the Rev. O. Fisher.

The 25th of July, 1855, on which the first and most severe shock was felt, was a very wet, close day, and the little wind stirring came from the S.W.

In the Münster Thal the earthquake began by a rumbling vibration like that caused by a carriage run under an archway, gradually increased for about four seconds, and then suddenly ceased. The oscillation seemed to be from E.S.E. to W.N.W., but would be affected by the build of the house.

In the church at Brienne two stones fell from the groining thirty or forty feet into the organ pipes, to a point between 2 and $2\frac{1}{2}$ feet N. by E. of the point vertically beneath their first position; and allowing for the direction of the building, this would give the motion of the earth about from N.E. to S.W. This wave may have been a reflexion caused by the wave entering the Jura from the valley. Another shock was felt at Brienne, at 10 A.M., on the 26th.

The great shock was felt at Strasburg, slightly at Lyons in a direction from E. to W.; likewise at Chambery, Alessandria, and Genoa. The account given by Plana in the *Times* does not seem very intelligible, but as far as can be made out from the stopping of the clocks, it gives the direction of the shock at Turin about 30° W. of S. Chiavenna, the western shore of the Lake of Constance, and

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Schaffhausen seem to fix the limit to which it was felt towards the east. The area shaken was therefore an oval, having its largest dimension about 300 miles N. and S., and its shortest 250 miles E. and W.

At Geneva the shock appeared to be directed to E.N.E. At Thun it appeared to come from Frutigen. At Kandersteg, at the north foot of the Gemmi, the shock was N. and S. At Interlaken the shocks were more severe; and at Ormont, Canton Vaud, the oscillation came from W. to E., preceded by a noise which lasted for an instant only, and the roof of a house fell in. It seems that nearer the centre of the oval the intensity of the shock was greater. At the baths of Leuk a chimney was thrown down and the walls cracked; but on ascending the valley of the Rhone the evidence of disturbance became rapidly more marked up to Visp, where only seven houses remained habitable. At the little inn, the "Soleil," the flag pavement was burst upwards as if by a blow from beneath: a continual succession of shocks have occurred there at variable intervals up to the present time. Passing on towards Brieg, the evidence of the violence of the shock rapidly diminished. The valley of Zermatt showed the chief disturbance; the bridle road was continually fissured, and in some places slipped down into the valley. At Stalden there was much destruction, but at St. Nicholas the havoc was very great indeed. Higher up the traces of the shock were less and less, until at Tesch, Randa, and Zermatt, there was no mischief done. The other branch of the valley by Saas did not suffer so much.

Drawing lines through the different places in the direction in which the wave proceeded, it will be found that they converge very nearly to Visp, showing that to be nearly the centre of disturbance.

Mr. Croker of Caius College was walking between Stalden and Visp when the great shock occurred, which appeared to him to be a blow from beneath like the springing of a mine under him, and he observed that the path sunk several inches from the solid rock; a lofty isolated rock on the opposite side of the valley vibrated, and blocks of stone came tumbling down on all sides. The quivering lasted about thirty seconds. He did not observe any sound preceding the shock, though this was heard at Visp; but a crashing sound accompanied the great shock, and a fainter sound continued afterwards beyond the motion. He felt continued shocks from one o'clock till four, when he proceeded towards Sion. At Zermatt the same shock was felt very much less violently, and no sound preceded it; and after attaining its maximum, it ceased somewhat suddenly. It was felt less strongly on the Riffelberg; and on the 27th another, felt at Zermatt as strongly as before, was not felt on the little Mont Cervin.

A sound seems in general to have preceded the earthquake at places near the centre of disturbance: at Visp likened to the echo of an avalanche, but at a distance there was only a sound simultaneous with the shock. The sound may have arisen from the grinding of the walls of the fissure, or whatever violent action may have occurred at the origin, and the sound-waves travelling more rapidly than the earthquake-wave. This is opposed to Mr. Mallet's view,

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though he gives a table in which the least rate given for sound travelling through any kind of stone is 3640 feet per second, while the rate of motion of the earthquake of Lisbon was 1750 feet. If the view stated be correct, the disturbance must have been deep in the earth, which would also explain the upward blow felt by Mr. Croker. At greater distances the sound-wave would be expended sooner than the earthquake-wave, and the accompanying sound be due to local action.

Near the centre the shock was sudden, passing away gradually. At a distance it began with slight quivering, gradually attained a maximum, and then suddenly ceased. Now if the disturbance occurred along a large fissure, perhaps several miles in length, and of unknown depth, the waves from different portions would reach any given point in succession, and at intervals the combined effect of many waves would be felt, producing a result analogous to the rolling of thunder due to the varying distance of the source of sound, while the sudden concussion at a nearer point is like the detonation heard when the lightning is near the auditor.

The shocks were less severe in the mountains than in the valleys. As far as the wave progressing horizontally is concerned, it would, on entering a mountain, at first be nearly bounded by a horizontal plane continuous with that of the valley, just as light is propagated in straight lines; but there would also be a diversion of a part (analogous to the diffraction of light at a screen) into the mountain, so that where the wave passed for some distance into a range it would finally be felt at the summit. It is observable that the shock on the 25th was less severe on the Riffelberg than at Zermatt, yet it travelled through the mountain and was felt at Turin.

The period of elevation of the Alps seems about contemporaneous with the older Pliocene of Sir C. Lyell. The country is broken up with faults, which probably there, as elsewhere, follow the lines of valleys. The valley of Visp lies in the axis of two ranges which have all the appearance of a mighty valley of elevation. The shock may have arisen from a shifting of the beds on this line of ancient disturbance, and very probably the somewhat rectangular corner between the valleys at Visp suffered the principal displacement. Earthquakes in non-volcanic regions probably arise from a failure of support. At the period of the elevation of the Alps, the more heated lower parts of the earth's crust must have come nearer to the surface than their normal position, and contractions and failure of support must occur while cooling, and the comparatively recent elevation of the Alps may give reason for thinking this to be still going on.

XXX. *Intelligence and Miscellaneous Articles.*

ON THE DIRECTION OF THE VIBRATIONS OF THE ÆTHER IN THE CASE OF POLARIZED LIGHT. BY M. HAIDINGER.

HAIDINGER has made a communication from Stokes the occasion of an interesting examination of the long-mooted question whether the vibrations of the æther take place in the plane of polarization or at right angles to it. The former opinion it will be

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remembered was held by Maccullagh and Neuman, and at one time by Cauchy; the latter is the view taken by Fresnel, Cauchy, Beer and the majority of physicists who have written upon the subject. Our readers will remember that the question, considered from the mathematical point of view, amounts to this. Is the density of the æther to be considered constant and its elasticity variable; or is the elasticity to be considered constant and the density variable? The former supposition leads to the conclusion that the vibrations are at right angles to the plane of polarization; the latter that they are in this plane. It is only an appeal to experiment which can decide the question, or rather it is only this appeal which can throw the weight of probability upon the one side or the other. Haidinger supports Fresnel's view, and bases his reasoning upon the phænomena of pleochroism in doubly refracting crystals. We shall simply translate the author's succinct expression of his own argument.

I. Let the object be a dichroöus crystal and let equal thicknesses of its substance be investigated.

II. The following positions are considered as demonstrated:—

a. The vibrations of the luminiferous æther are transverse.

b. To the same colours belong equal wave-lengths; to different colours different wave-lengths.

III. Mode of investigation.

(1.) *Observation.*—In the horizontal zone (of a uniaxial crystal) whose edges are parallel to the axis in all azimuths, one ray or bundle of rays (an image of the dichroscopic lens or of any doubly refracting prism), viz. the ordinary ray, is polarized parallel to the axis with the colour A, and one ray, or bundle of rays, the extraordinary ray, is polarized perpendicular to the axis with the colour B.

Inference.—The vibrations are either perpendicular to the plane of polarization or in this plane.

Hypothesis.

1. The vibrations are perpendicular to the plane of polarization.	2. The vibrations are in the plane of polarization.

Consequences.

1. The direction of the vibrations of the ordinary ray is perpendicular to its plane. There are an infinite number of such directions; they are perpendicular to the axis.	1. The direction of the vibrations of the ordinary ray lies in its plane. For all azimuths there is but one such direction of vibration. It is in the direction of the axis.
2. To one colour A or wave-length belongs an infinite number of directions of vibration, but in as many different planes of polarization.	2. To one colour A or wave-length belongs only one direction of vibration.
3. To an infinite number of	3. To an infinite number of

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planes of polarization belongs an infinite number of directions of vibration. The directions are perpendicular to each plane.

4. The direction of vibration of the extraordinary ray is perpendicular to its plane. There is but one such direction; it is parallel to the axis.

5. A colour B, that is a wave-length, is in all azimuths united to one direction of vibration.

6. To one plane of polarization belongs one direction of vibration.

planes of polarization belongs but one direction of vibration.

4. The direction of vibration of the extraordinary ray lies in the plane of polarization. There is an infinite number of such directions. They lie in all azimuths perpendicular to the axis.

5. A colour B is united to an infinite number of directions of vibration, one in each azimuth.

6. To one plane of polarization belongs an infinite number of directions of vibration.

(2.) *Observation.*—In the vertical zones whose edges are perpendicular to the axis of the crystal, in all azimuths, the ordinary ray is polarized in the direction of the axis with the colour A. The extraordinary ray is polarized perpendicular to the axis, and goes from the direction of the observation, beginning perpendicular to the axis, to the direction of the axis itself, passing from the colour B to the colour A. Observed in the direction of the axis, the colours of both rays perpendicular to each other are perfectly similar in all azimuths, and possess the tone A.

Consequences and Hypotheses as above.

7. The direction of vibration of the ordinary ray is perpendicular to its plane. There is but one such direction for every plane. It is perpendicular to the axis.

8. To one colour or wave-length belongs but one direction of vibration.

9. The direction of vibration of the extraordinary ray is perpendicular to its plane of polarization. There is in every principal section an infinite number of such directions between 0° parallel to the axis and 90° perpendicular to the axis.

10. To the succession of colours or wave-lengths from B to A belongs an infinite number of directions of vibration inclined from 0° to 90°.

7. The direction of vibration of the ordinary ray lies in its plane. There is an infinite number of such directions for every plane. They include with the axis all possible angles from 0° to 90°.

8. To one colour or wave-length belongs an infinite number of directions of vibration.

9. The direction of vibration of the extraordinary ray is in its plane of polarization. There is for every principal section only one such direction. It is perpendicular to the axis.

10. To the whole series of colours from B to A belongs, notwithstanding the different wave-lengths, but a single direction of vibration.

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(3.) *Combination of the observations and conclusions in* III., 1. *and* III., 2.

11. The same direction of vibration is connected with the same tone of colour or the same wave-length.

12. In the direction of the axis we do not see the colour B because the direction of vibration belonging to this colour has a longitudinal position.

13. The constant (or limiting) tones of colour A and B are connected with vibrations; B in the direction of the axis, A perpendicular to it.

14. In the direction of the axis we see the colour A by vibrations perpendicular to the axis. In a direction perpendicular to the axis we also see the same colour A by vibrations perpendicular to the axis.

15. Vibrations perpendicular to the axis take place only for the colour A.

16. Vibrations in the direction of the axis take place only for the colour B. This colour is therefore invisible in the direction of the axis.

17. For the colour A the vibrations take place only perpendicular to the axis.

18. In the mixed tones of colour, each colour appears, according to its appropriate direction of vibration, dependent on the cosine of the inclination of the last with the usual ray.

11. The same direction of vibration is connected with the same tone of colour, only perpendicular and parallel to the axis. In all other directions it is connected with all possible gradations of colour.

12. In the direction of the axis we do not see the colour B, although the vibrations belonging to it take place in all azimuths perpendicular to the axis.

13. The constant tones of colour A and B are connected with vibrations; B perpendicular to the axis, A parallel to it, perpendicular to it, and making all intermediate angles with it.

14. In the direction of the axis we see the colour A by vibrations perpendicular to the axis. In a direction perpendicular to the axis we see the same colour A by vibrations parallel to the axis.

15. Vibrations perpendicular to the axis take place for A, B, and every intermediate colour.

16. Vibrations perpendicular to the axis take place for the colour B, notwithstanding this colour is invisible in the direction of the axis. Just such vibrations, however, take place for the colour A, and yet this colour is visible in the direction of the axis.

17. For the colour A, the vibration takes place in all azimuths perpendicular to the axis, in all azimuths along the axis, and in all azimuths of the principal section.

18. Mixed colours occur without a change in the direction of vibration.

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19. The same direction of vibration belongs to the colour A when the observation is in the direction of the axis or perpendicular to it.	19. To the colour A belong, when the observation is in the direction of the axis, vibrations perpendicular to the axis. When the observation is perpendicular to the axis, the vibrations are in the direction of the axis and perpendicular to all those of the last case. Yet there is no trace of any action on the part of the first set.
20. For the same direction of vibration and the same wave-length there is the same colour throughout the whole crystal.	20. For the same direction of vibration there are different colours and therefore different wave-lengths.

The author concludes from this reasoning, that the assumption of vibrations perpendicular to the plane of polarization leads to clear, simple, consequent, and connected views of the whole subject; while the opposite supposition involves obscure, overloaded, and contradictory representations. Similar arguments may be drawn from a consideration of biaxial or trichromatic crystals. We must, however, refer to the original paper in Poggendorff's *Annalen* for October 1855, for a fuller exposition of the author's views and arguments.—*Silliman's Journal* for January 1856.

ON THE INCANDESCENCE OF METAL WIRES IN ALCOHOLIC VAPOUR. BY H. REINSCH.

Reinsch long since showed that wires of all the infusible metals, as also most of the metallic oxides, continued incandescent in the vapour of alcohol, so that this property is by no means peculiar to platinum. He has had the opportunity of making some experiments upon this phænomenon, which have led him to a remarkable observation. When a spiral of copper wire is fastened in the manner described by Reinsch upon the wick of a spirit-lamp, and the lamp is lighted and then quickly blown out, the copper wire continues incandescent just as well as platinum. This incandescence, however, only lasts two to three minutes. But if a small fragment of coke be inserted into the spiral and the whole be brought to a red heat, the wire will continue to glow permanently after the extinction of the flame. The author attempted to explain this phænomenon by the supposition that the high temperature was better maintained by the fragment of coke, as it does not carry off the heat so quickly as the wire, and as it were forms a sponge, which always retains sufficient heat to communicate to the wire the temperature necessary for its incandescence. This supposition, however, was not confirmed, as the spiral was not extinguished even when the piece of coke was removed.

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If it be then blown out and again ignited, it continues to glow without the piece of coke, so that by contact with the coke it has passed into a peculiar state, which, in the author's opinion, must arise from an electrical action between the two substances.—*Archiv der Pharm.*, cxxxiv. p. 187.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1856.

Chiswick.—January 1. Overcast: cloudy. 2. Foggy: exceedingly fine: slight rain. 3. Fine: uniformly overcast: fine. 4. Cloudy and mild: overcast: rain at night. 5. Rain: densely overcast: rain. 6. Cloudy: rain. 7. Cloudy: foggy: cloudy. 8. Densely overcast: cloudy and cold. 9. Drizzly: rain and sleet: rain at night. 10. Cloudy and cold: clear and frosty. 11. Frosty: cloudy: frosty. 12. Cloudy: sunshine occasionally: cloudy. 13. Clear and frosty: fine. 14. Dry and frosty. 15. Sharp frost: clear: fine. 16. Fine: rain and fog: cloudy. 17. Overcast: cloudy: rain. 18. Slight rain: cloudy. 19. Rain: heavy clouds. 20, 21. Densely overcast: heavy clouds: slight rain. 22. Densely clouded: rain. 23. Low clouds: bright sun at intervals: cloudy and windy. 24. Densely clouded and boisterous: rain: lightning at night. 25. Overcast: fine. 26. Very fine: rain. 27. Fine: frosty. 28. Frosty: overcast: hail-shower: fine. 29. Clear and frosty: fine: sharp frost. 30. Frosty: fine: cloudy and cold: frosty. 31. Clear and frosty: cloudy: frosty.

Mean temperature of the month	38°·43
Mean temperature of Jan. 1855	33 ·45
Mean temperature of Jan. for the last thirty years	36 ·94
Average amount of rain in Jan.	1·690 inch.

Boston.—January 1. Cloudy. 2. Foggy. 3. Fine. 4. Cloudy: rain P.M. 5. Cloudy: rain A.M. 6. Fine. 7. Cloudy. 8. Cloudy: rain P.M. 9, 10. Fine. 11. Fine: snow A.M. 12—16. Fine. 17, 18. Cloudy: rain P.M. 19. Rain A.M. 20. Cloudy: rain P.M. 21. Rain A.M. and P.M. 22. Fine. 23. Rain A.M. 24. Cloudy: rain A.M. 25. Fine. 26. Cloudy: rain A.M. 27. Fine. 28. Fine: rain and snow P.M. 29. Fine. 30, 31. Cloudy.

Sandwick Manse, Orkney.—January 1. Cloudy A.M. and P.M. 2. Damp A.M. and P.M. 3. Cloudy A.M.: damp P.M. 4. Cloudy A.M. and P.M. 5. Damp A.M.: rain P.M. 6. Damp A.M. and P.M. 7. Rain A.M. and P.M. 8. Sleet-showers A.M. hail-showers P.M. 9. Snow-showers A.M.: clear, frost P.M. 10. Snowing A.M.: snow-showers P.M. 11. Snow-showers A.M.: snow-drift P.M. 12. Bright A.M.: thaw, showers P.M. 13. Cloudy A.M. and P.M. 14. Cloudy A.M.: fine, cloudy P.M. 15. Cloudy, frost A.M.: fine, cloudy P.M. 16. Cloudy A.M.: fine, cloudy P.M. 17. Damp A.M.: rain P.M. 18. Clear A.M.: fine, clear P.M. 19. Frost A.M.: rain P.M. 20. Bright A.M.: hail-showers P.M. 21. Bright, frost A.M.: clear, frost P.M. 22. Clear, frost A.M.: clear P.M. 23. Sleet A.M.: clear, fine P.M. 24. Cloudy A.M.: cloudy, fine P.M. 25. Cloudy A.M.: showers P.M. 26. Showers A.M.: sleet-showers P.M. 27. Clear A.M.: showers P.M. 28. Showers A.M.: snow-showers P.M. 29. Snow-showers A.M. and P.M. 30, 31. Bright A.M.: snow-showers P.M.

Mean temperature of Jan. for twenty-nine previous years	38°·38
Mean temperature of this month	38 ·00
Mean temperature of Jan. 1855	38 ·16
Average quantity of rain in Jan. for fifteen previous years	4·24 inches.

The remarkable depression of the barometer here on the 23rd, 24th and 25th is worthy of observation, coupled with the fact, that the gale which it indicated did not reach Orkney, or the N. of Scotland, while it was violent in England. The first two of these days were really fine here, and marked so in the Register.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1856. Jan.	Max.	Min.		9½ a.m.	8½ p.m.	Max.	Min.		9½ a.m.	8½ p.m.						
1.	29·872	29·793	29·62	29·62	29·64	46	39	37	43	41	s.	sse.	se.	.....	.....	.....
2.	29·659	29·517	29·38	29·62	29·54	50	36	39	42	43	se.	s.	sse.	·01	.....	·04
3.	29·611	29·531	29·28	29·57	29·58	49	39	39	43	43½	s.	s.	se.	·04	.....	.....
4.	29·647	29·600	29·36	29·58	29·60	47	40	41	44½	42	se.	s.	se.	·07	.....	·08
5.	29·520	29·421	29·22	29·55	29·49	49	43	41	43½	43	se.	s.	e.	·11	·58	.....
6.	29·310	29·174	29·04	29·48	29·43	48	35	41	43	43	e.	e.	ene.	·12	.....	·09
● 7.	29·066	28·970	28·78	29·33	29·33	47	37	42	41	38	ne.	se.	e.	.....	.....	·09
8.	29·146	29·021	28·72	29·32	29·38	43	32	39·5	36	32	ne.	nnw.	n.	·02	.....	·09
9.	29·204	29·158	28·93	29·40	29·45	38	34	37	31½	31½	ne.	sse.	ne.	·23	·04	·04
10.	29·622	29·317	29·15	29·55	29·73	40	25	34	31	30	ne.	n.	ne.	.....	.....	.....
11.	30·071	29·786	29·59	29·95	30·20	38	28	26·5	29	33	ne.	nnw.	nw.	.....	·06	.....
12.	30·522	30·291	30·07	30·33	30·28	36	21	27·5	35	42	ne.	n.	w.	.....	.....	·35
13.	30·627	30·544	30·34	30·14	30·15	39	20	22·5	45	46½	ne.	sw.	wsw.	.....	.....	·13
☽ 14.	30·304	30·029	30·14	30·17	30·08	37	19	20	45	38	ne.	e.	s.	.....	.....	.....
15.	29·954	29·922	29·65	29·68	29·54	41	26	25	35	34	w.	ssw.	calm	.....	.....	.....
16.	29·916	29·839	29·64	29·58	29·56	42	35	32	42½	43	sw.	w.	wsw.	·01	.....	·03
17.	29·593	29·460	29·26	29·46	29·40	50	44	37	40½	37	s.	s.	nne.	·02	.....	·09
18.	29·461	29·417	29·08	29·42	29·52	51	38	45	44	33½	sw.	sw.	e.	·08	·05	·03
19.	29·250	29·192	28·97	29·35	29·13	51	44	42·5	35	36	sw.	se.	ne.	·10	·10	.....
20.	29·103	29·034	28·72	29·02	29·16	40	42	46	39½	34	s.	s.	nw.	·06	·26	·18
21.	29·071	29·026	28·76	29·32	29·40	51	37	42·5	35	35½	sw.	e.	w.	·36	·25	·03
○ 22.	29·515	29·296	29·02	29·46	29·42	42	37	36·5	33	35½	ne.	ne.	se.	·04	·60	.....
23.	29·344	29·220	28·87	28·88	28·81	54	45	39	34	37	sw.	s.	sse.	·12	·07	·05
24.	28·992	28·928	28·52	28·51	28·46	52	39	48·5	41½	39	sw.	s.	se.	·13	·05	·14
25.	29·432	29·998	28·63	28·49	28·80	50	35	39	41	41	w.	sw.	w.	.....	·04	.....
26.	29·473	29·398	29·10	29·06	29·27	51	26	38·5	42	40½	sw.	ssw.	w.	·14	·02	·14
27.	29·811	29·636	29·32	29·42	29·27	47	20	39	41	40	nw.	w.	w.	·02	.....	·07
28.	29·737	29·621	29·33	29·25	29·55	42	24	35	34	30	sw.	w.	n.	·08	.....	·29
29.	29·696	29·694	29·38	29·60	29·67	38	19	25·5	34	35	nw.	n.	n.	.....	·08	·17
☾ 30.	30·031	29·730	29·48	29·77	29·96	38	21	28	36	31	nw.	w.	n.	.....	.....	·13
31●	30·178	30·075	29·77	30·05	30·06	37	19	33	33	34½	nw.	n.	wnw.	.....	.....	·07
Mean.	29·636	29·536	29·26	29·481	29·511	44·64	32·22	36·0	38·50	37·51				1·76	2·20	2·33

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1856.

XXXI. *On Diamagnetic Action.* *By* F. REICH*.

IT is still a point of discussion whether the repulsion exercised by a magnetic pole upon a diamagnetic body is the effect of a polarity induced in the latter or not. Mr. Tyndall has recently founded a decision of the question on the consideration, that the diamagnetic repulsion must increase in the simple ratio of the strength of the current if it be simply an action of the magnetic pole upon the unchanged substance of the diamagnetic body; on the contrary, the increase must be as the square of the strength of the current if the action be due to an excited polarity; just as the action of one magnet upon another, whose magnetism is unchanged by the former, is simply as the current strength, whereas upon a piece of soft iron it is as the square of the magnetic intensity. Partly by his own experiments, and partly by others previously made by E. Becquerel, Mr. Tyndall has shown that the diamagnetic repulsion augments with the square of the current, which is a new proof of the polarity of a diamagnetic body.

I should not have thought of subjecting these experiments to a corroborative repetition, were I not called upon by M. Matteucci to do so, by means of the torsion balance which I had constructed for the determination of the density of the earth; and as the experiments have now been made, I hope that their publication will not be altogether without interest.

On one end of the torsion balance hangs a sphere of bismuth weighing 484·15 grms., surrounded by a cylindrical wooden chamber coated within and without with tinfoil. On a level

* From Poggendorff's *Annalen*, vol. xcvii. p. 283.

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with the centre of this sphere a magnet of known strength was placed at a certain distance from the mass of bismuth, and the consequent repulsion was observed. The arm of the torsion balance remained during the experiment in a state of oscillation, which at the commencement could be produced at will, either by the attraction of a mass of lead, or by the diamagnetic action itself*.

From the experiments on the density of the earth, it is known that the oscillating torsion balance never retains for any length of time the same position of equilibrium unchanged, even where all external influences are, as far as we know, preserved constant. These alterations are indeed inconsiderable, but still of a nature calculated to vitiate experiments like the present. Other sources of disturbance the statement of the experiments themselves will reveal.

First experiment.—Three square bar-magnets, 496 millims. long and 8·6 millims. thick, characterized by the numbers 4, 5, and 6, were permitted, for the purpose of ascertaining their intensity, to act from a distance of $0^m{\cdot}5$ upon a compass-needle. The deflection by No. 4 alone was found to be $9° 45'$; by Nos. 4 and 5 united, $18° 30'$; by Nos. 4, 5, and 6 united, $23° 24'$: the relation of these intensities therefore was as 1 : 1·9472 : 2·5789; and the relation of the squares of the intensities as 1 : 3·7917 : 6·6508.

These three magnets were brought in succession as close to the sphere of bismuth as possible, that is to say, into contact with the chamber of the torsion balance, and the following numbers were observed:—

Position of equilibrium	without magnet	59·200
Position of equilibrium	with magnet No. 4	55·550
...	... 4 & 5	48·750
...	... 4, 5 & 6	42·425
...	... 4 & 5	48·075
...	... 4	54·575
...	without magnet	57·500

It will be observed here that the position of equilibrium of the balance has sensibly changed; this alteration, however, is nearly proportional to the time. Taking, therefore, the mean, we obtain,—

The position of equilibrium	without magnet	58·3500
The position of equilibrium	with magnet No. 4	55·0625
...	... 4&5	48·4125
...	... 4,5&6	42·4250

* Der Arm der Torsionswaage blieb dabei immer im Schwingen, was zu Anfang beliebig durch die Attraction einer Bleimasse oder durch die diamagnetische Einwirkung selbst hervorgerufen wurde.

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The repulsions, therefore, exerted by the respective magnets were as follows:—

No. 4.	3·2875	divisions of the scale	$=0{\cdot}3840=1$ mm
... 4 & 5.	9·9375	...	$=1{\cdot}1608=3{\cdot}0228$
... 4, 5 & 6.	15·9250	...	$=1{\cdot}8601=4{\cdot}8441$

The ratio of these repulsions shows decidedly that they increase more speedily than in the simple ratio of the magnetic forces; but the ratio is still far from that of the squares of the forces. The principal cause of this divergence is, that the distance of the sphere of bismuth from the magnetic pole increased with the repulsion; and this augmentation of distance must exert a considerable influence, both because the total distance was but small, and also because, supposing diamagnetic polarity to exist, the repulsion must be in the inverse ratio of the fourth power of the distance. Denoting, therefore, by C the distance of the centre of repulsion in the bismuth sphere in its position of equilibrium without the magnet, from the place occupied by the magnetic pole, then in the case of No. 4 we have the distance equal $C+0{\cdot}3840$; with Nos. 4 and 5, $C+1{\cdot}1608$; and with Nos. 4, 5 and 6, $C+1{\cdot}8601$: the repulsive forces, therefore, are to each other in the ratio

$$\frac{1}{(C+0{\cdot}3840)^4} : \frac{3{\cdot}7917}{(C+1{\cdot}1608)^4} : \frac{6{\cdot}6508}{(C+1{\cdot}8601)^4}.$$

By setting these ratios equal to the repulsions found by experiment, the quantity C, which is not to be found by direct measurement, might be determined; for this purpose, however, the experiments are not sufficiently exact. On account of the smallness of the distance from the sphere of bismuth, the lateral position, and consequently oblique action of the magnets, must make itself felt when three of them are employed.

Second experiment.—It is plain from the foregoing experiments, that it is more advantageous to permit the magnets to act from a greater distance upon the sphere. To obtain a sufficient repulsion under these conditions, the intensity of the magnets must be considerably increased. For this purpose thirty-two bar-magnets, quite similar to those described, were made use of. They were characterized by successive numbers, and lay, when they all acted together, in four horizontal series, each embracing eight bars; so that from Nos. 1 to 8 formed the first quarter, from Nos. 2 to 16 the second quarter, &c. of the surface formed by the ends of the bars: the centre of this surface was in the horizontal line passing through the centre of the sphere at right angles to the arm of the torsion balance, and at a distance of 50 millims. from the chamber. I determined the respective inten-

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sities of the bundles by the deflection of a compass-needle placed at a metre distance, and obtained as follows:—

Nos. 1 to 8,	a deflection of	11°	3′,	intensity	1
... 1 ... 16,	...	15	45,	...	1·4442
... 1 ... 24,	...	21	42,	...	2·0378
... 1 ... 32,	...	27	3,	...	2·6148

The relation of the squares of these intensities is therefore expressed by 1 : 2·0857 : 4·1525 : 6·8369. The position of equilibrium of the torsion balance was then observed:—

Without magnet	beginning	57·875	mean	58·9000
	end	59·925		
By Nos. 1 to 8	beginning	55·125	...	55·7250
	end	56·325		
... 1 ... 16	beginning	51·975	...	51·9875
	end	52·900		
... 1 ... 24	beginning	46·300	...	46·5125
	end	46·725		
... 1 ... 32	beginning	39·625	...	39·5725
	end	39·525		

From this we obtain the repulsion,—

Nos. 1 to 8	3·1750	divisions	= 0·3709	millims.	= 1
... 1 ... 16	6·9125	...	= 0·8074	...	= 2·1772
... 1 ... 24	12·3875	...	= 1·4470	...	= 3·9016
... 1 ... 32	19·3250	...	= 2·2573	...	= 6·0866

This ratio of the repulsions does not differ more from that of the squares of the magnetic strengths than may be referred to the increase of distance with the repulsion; for assuming for C the not improbable value of 70 millims., we obtain the ratio of the repelling forces to be—

$$\frac{1}{(70{\cdot}3709)^4} : \frac{2{\cdot}0857}{(70{\cdot}8074)^4} : \frac{4{\cdot}1525}{(71{\cdot}4470)^4} : \frac{6{\cdot}8369}{(72{\cdot}2573)^4}$$

$$= 1 : 2{\cdot}0347 : 3{\cdot}9080 : 6{\cdot}1504,$$

which does not differ from the ratio of the observed repulsions more than may be accounted for by the unavoidable errors of observation.

Third experiment.—As the application of an electro-magnet permitted us to hope for more exact results, because, while the position of the magnet remains wholly unchanged, it is in our power to change and measure its strength at pleasure, I placed a round bar of iron, 32 millims. thick and 440 millims. long, contained within a spiral of thick copper wire, horizontally near the sphere of bismuth, so that its nearest end was 67 millims. from the chamber of the torsion balance, and permitted a cur-

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rent of from 1 to 4 of Daniell's elements to pass through the spiral. In the circuit a tangent-compass was introduced, and from two fixed points of the circuit a branch current was sent through the multiplier of a sine-compass. The tangent-compass was only divided into single degrees, and admitted therefore of no very accurate measurement of the strength of the current. The sine-compass, constructed by Œrtling of Berlin, is a very perfect instrument, and permitted of a safe observation of the deflection of the needle to single minutes. It was, however, found that after the circuit was broken the needle did not return exactly to zero, a consequence of the construction which materially interferes with the exactitude of the instrument. The observations gave as follows:—

	Position of equilibrium.	Tangent-compass.	Sine-compass.
Without current ...	83·300	0°0	0° 0′
1 element	80·650	16·1	2 41
2 elements	75·575	25·5	4 30
3	71·175	32·5	5 52
4	67·625 67·650	36·4	6 51
3	72·100	31·6	5 46
2	76·625	25·2	4 31
1 element	80·775	15·7	2 43
Without current.....	82·975	0·0	0 10

We have therefore as mean result,—

No. of elements.	Position of equilibrium.	Repulsion.			Strength of current.						
					Tangent-compass.			Sine-compass.			
		Divisions.	Millims.	Ratio.	Deflection.	Ratio.		Deflection,		Ratio.	
					Observed.	Simple.	Square.	Observed.	Corrected.	Simple.	Square.
0	83·1375	0	0	0	0°0	0	0	0° 5′	0° 0′	0	0
1	80·7125	2·4250	0·2833	1	15·9	1	1	2 42	2 37	1	1
2	76·1000	7·0375	0·8220	2·9020	25·35	1·6743	2·8031	4 30·5	4 25·5	1·6900	2·8561
3	71·6375	11·5000	1·3433	4·7423	32·05	2·2125	4·8953	5 49	5 44	2·1881	4·7880
4	67·6375	15·5000	1·8105	6·3918	36·4	2·6054	6·7883	6 51	6 46	2·5809	6·6609

Fourth experiment.—In the arrangement nothing was changed, except that the tangent-compass and sine-compass were placed in another room, so as to avoid all attraction of the electro-magnet upon the instruments. M. Fritzsche had the kindness to lend me his assistance in making the observations. We observed as follows:—

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	Position of equilibrium.	Tangent-compass.	Sine-compass.
Without current ...	84·275	0°·0	0° 0′
1 element	80·825	13·0	2 46
2 elements	75·500	21·9	4 20
3	70·250	27·8	5 38
4	66·225	31·6	6 30
	66·350		
3	70·875	26·6	5 18
2	75·450	19·6	4 0
1 element	79·075	12·0	2 38
Without current.....	81·575	0·1	0 30

Hence as mean result:—

No. of elements.	Position of equilibrium.	Repulsion.			Strength of current.						
					Tangent-compass.			Sine-compass.			
		Divisions.	Millims.	Ratio.	Deflection.	Ratio.		Deflection.		Ratio.	
					Observed.	Simple.	Square.	Observed.	Corrected.	Simple.	Square.
0	82·9250	0	0	0	0°·05	0	0	0° 15′	0° 0′	0	0
1	79·9500	2·9750	0·3475	1	12·5	1	1	2 42	2 27	1	1
2	75·4750	7·4500	0·8702	2·5042	20·75	1·7115	2·9293	4 10	3 55	1·5979	2·5532
3	70·5625	12·3625	1·4440	4·1555	27·2	2·3228	5·3957	5 28	5 13	2·1337	4·5950
4	66·2875	16·6375	1·9434	5·5924	31·6	2·7811	7·7343	6 30	6 15	2·5467	6·4859

Fifth experiment.—With a view of obtaining greater deflections, the current conducted through the sine-compass was caused to branch from two points of the circuit more distant from each other than the former ones. It was, however, found that the distance between the points from which the branch current was conducted was too great to permit of the stronger currents being read off upon the sine-compass. In these experiments, also, I have to acknowledge the assistance of M. Fritzsche. We obtained,—

	Position of equilibrium.	Tangent-compass.	Sine-compass.
Without current.....	84·975	0°·0	0° 0′
1 element	82·850	13·0	24 44
2 elements	78·225	20·8	49 30
3	73·775	26·0	not observed.
4	68·8125	30·2	not observed.
3	73·725	24·9	79 24
2	78·525	19·2	45 26
1 element	82·475	11·8	24 9
Without current.....	84·700	0·8	0 20

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Hence as mean result:—

No. of elements.	Position of equilibrium.	Repulsion.			Strength of current.						
					Tangent-compass.			Sine-compass.			
		Divisions.	Millims.	Ratio.	Deflection.	Ratio.		Deflection.		Ratio.	
					Observed.	Simple.	Square.	Observed.	Corrected.	Simple.	Square.
0	84·8375	0	0	0	0°·4	0	0	0° 10′	0° 0′	0	0
1	82·6625	2·1750	0·2541	1	12·4	1	1	24 26·5	24 16·5	1	1
2	78·3750	6·4625	0·7549	2·9713	20·0	1·6752	2·8064	47 33	47 23	1·7900	3·2041
3	73·7500	11·0875	1·2951	5·0977	25·05	2·1988	4·8347	79 14	79 4	2·3896	5·7101
4	68·8125	16·0250	1·8717	7·3678	29·8	2·6944	7·2596				

Sixth experiment.—The distance between the points of the circuit from which the current of the sine-compass branched off was less than in the fifth, and greater than in the fourth experiment. The following are the observations:—

	Position of equilibrium.	Tangent-compass.	Sine-compass.
Without current ...	80·1625	0°·0	0° 0′
1 element	77·100	11·6	15 10
2 elements	72·300	20·0	27 17
3	67·925	25·0	36 18
4	64·275	28·2	42 52
3	69·800	23·3	33 28
2	93·775	18·3	25 9
1 element	76·675	11·0	14 45
Without current.....	78·225	0·0	0 10

Hence as mean result we have,—

No. of elements.	Position of equilibrium.	Repulsion.			Strength of current.						
					Tangent-compass.			Sine-compass.			
		Divisions.	Millims.	Ratio.	Deflection.	Ratio.		Deflection.		Ratio.	
					Observed.	Simple.	Square.	Observed.	Corrected.	Simple.	Square.
0	79·1937	0	0	0	0°·0	0	0	0° 5′	0° 0′	0	0
1	76·8825	2·3112	0·2700	1	11·3	1	1	14 57·5	14 52·5	1	1
2	73·0375	6·1562	0·7191	2·6636	19·15	1·7378	3·0202	26 13	26 8	1·7118	2·9439
3	68·8625	10·3312	1·2067	4·4700	24·15	2·2439	5·0350	34 53	34 48	2·2232	4·9425
4	64·2750	14·9187	1·7426	6·4550	28·2	2·6854	7·2006	42 52	42 47	2·6459	7·0007

All the observations show that the repulsion augments in a far quicker ratio than the strength of the current, and that the

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ratio does not differ much from that of the squares of the currents. In general, however, the latter ratio is not quite attained, which is explained by the fact that the distance increases with the repulsion. We are therefore certainly justified in assuming that the diamagnetic repulsion is proportionate to the square of the magnetic intensity which produces the repulsion, and is therefore a product of a polarity excited by influence in the diamagnetic body.

With reference to the concluding paragraph of M. Reich's interesting paper, I may be permitted to express the opinion that the result which he has corroborated is not, in itself, a proof that the diamagnetic force is a polar force. It undoubtedly proves that the repulsion is due to a state of excitement into which the bismuth is thrown by the influencing magnet; but it does not, I think, prove the *dual* or *polar* nature of that excitement. True, we have no example in nature of a similar excitement that is not polar; but such is at least conceivable, although as yet unobserved. For this reason I thought it necessary, in the paper to which M. Reich has referred, to show that the excitement of diamagnetic bodies is of a duplex character; and that the attractions and repulsions of a bar of iron under magnetic influence, have their exact antitheses in the repulsions and *attractions* of a bar of bismuth acted upon by the same forces.

In repeating experiments such as those described in the paper to which M. Reich has alluded, a certain acquaintance with the delicate mechanical conditions required to be fulfilled is necessary to success. Knowing this, it was not a matter of surprise to me to learn that M. Matteucci failed, for a time, to realize my results, and was consequently led to doubt their existence. Hence, I suppose, his desire to see my experiments repeated with M. Reich's apparatus. The doubts referred to, I am happy to say, no longer exist, as through the kindness of M. Matteucci himself I learn that he has recently arrived at a complete corroboration of my experiments.

I may further remark, that in a paper now in the course of printing for the Philosophical Transactions, the conditions laid down by M. Matteucci for the "rigorous demonstration of diamagnetic polarity" are strictly fulfilled; so that, as far as I am aware, no single objection that has ever been made against *the fact* of diamagnetic polarity remains at the present time unanswered. Finally, with reference to the bearing of this question upon that of magnecrystallic action, I would refer the reader to a paper in the last Number of this Journal.

J. T.

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XXXII. *Chemical Examination of certain Lakes and Springs on the Turko-Persian frontier near Mount Ararat. By* HENRY M. WITT, *F.C.S., Assistant Chemist to the Government School of Science applied to Mining and the Arts**.

THE samples of these waters were brought over by William Kennett Loftus, Esq., F.G.S., and collected by him during the progress of a joint Commission appointed by the English, Russian, Turkish, and Persian Governments for the demarcation of the Turko-Persian frontier; they were placed in my hands for chemical examination in June last by Sir Roderick Murchison.

Unfortunately the samples were so small, only a wine quart bottle of each, that a complete chemical analysis was impossible, but the few experiments which I have been able to make suffice to show the interesting character of these lakes and springs.

Lake of Urumia.

Probably the most interesting is the Lake of Urumia, which Mr. Loftus states† to be "about 82 miles in length and 24 wide, its height being 4100 feet above the level of the sea. The water is of a deep azure colour, but there is something exceedingly unnatural in its heavy stillness and want of life. Small fragments of Fuci, saturated with salt, and thrown ashore, form a ridge at the margin of the lake, and emit such a noxious effluvium under a hot sun as to produce nausea at the stomach. The sulphuretted hydrogen generated from the lake itself without doubt adds to this sensation. The water is intensely salt, and evaporates so rapidly, that a man, who swam in to bring me a bottle of the water for analysis, on coming out was covered with particles of salt, and looked as white and ludicrous as though he had been thrown into a flour tub."

The sample was collected from the lake at Guverjin Kalah, on the north-western shore, on the 14th of August, 1852, the temperature of the water at the time being 78° F. at 11 A.M.

As I received it (the cork having been well secured by a coating of wax), the water still retained a strong smell of sulphuretted hydrogen, and was moreover supersaturated with carbonic acid, which it evolved on being shaken or gently heated. It was evidently a very strong brine, for it tasted intensely of common salt, left on every place on which a drop evaporated spontaneously a large quantity of saline residue; and by leaving a portion of it for a few hours in a warm laboratory in an open dish, large cubical crystals, exhibiting the peculiar step-like cavernous structure of common salt, separated in abundance.

* Communicated by the Author.

† On the Geology of portions of the Turko-Persian Frontier, by W. K. Loftus, Esq. (Communicated to the Geological Society from the Foreign Office, by order of the Earl of Clarendon; and published in the Quarterly Journal of the Society, vol. xi. p. 247.)

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Its specific gravity was 1·18812, and on evaporation it gave a total quantity of solid residue amounting to 21856·5 *grains in the gallon*!

In the imperial gallon (of 70,000 grains) there were present 10470·439 grains of chlorine, corresponding to 17254·27 grains of common salt; the remainder of the saline matter, amounting to 4602·23 grains, consisted chiefly of alkaline carbonates, but also contained small quantities of the sulphates and carbonates of lime and magnesia; the smallness of the quantity of water in my possession prevented the possibility of determining their actual amount.

To indicate the position of the Lake of Urumia among natural brines, I append a Table showing the specific gravities, total quantities of solid residue, and of common salt, in the gallon of several of the mineral springs of Harrogate (analysed by my friend Mr. Northcote and myself for, and under the direction of, Dr. Hofmann), as well as of other brine-springs, and the waters of certain seas.

Name of water.	Specific gravity.	Total residue in the gallon, in grains.	Common salt in the gallon, in grains.	Authority.
Seas:—				
The Mediterranean	.....	2870	.....	Pfaff, 1839*.
do.	.....	2851	1905	Laurens, 1839†.
do.	1·0287	.....	.....	A. H. and R. Schlagintweit, 1854‡.
English Channel	.....	2660	.....	Pfaff.
do.	.....	2468	1890	Schweitzer, 1839†.
German Ocean at the Frith of Forth	.....	2174	.....	Pfaff.
Baltic Sea at Kiel in Holstein	.....	1400	.....	do.
Atlantic	1·027	.....	.....	A. H. and R. Schlagintweit.
Red Sea	1·0315	.....	.....	do.
Brines:—				
Harrogate springs.				
1. Old sulphur well	1·01113	1096	866	Hofmann, 1854§.
2. Montpelier strong sulphur well	1·01045	966	803	do.
3. Hospital strong sulphur well	1·00515	437	369	do.
Dead Seas	1·211	17220	.....	Marcet‖.
Droitwich brine	1·1893	20157	19392	A. B. Northcote, 1855¶.
Stoke brine	1·2044	22256	21492	do.
Lake of Urumia	1·18812	21856	17254	H. M. Witt, 1856.

* Pfaff, Schwartze's *Allgemeine und specielle Heilquellenlehre*. Leipsic, 1839.

† Laurens and Schweitzer, Phil. Mag. Ser. 3. vol. xv. p. 51.

‡ Phil. Mag. for 1855, vol. ix. p. 396, "On the Temperature and Density of the Seas between Southampton and Bombay."

§ Hofmann, Quart. Journ. of Chem. Soc. vol. vii. p. 161.

‖ Marcet, Nicholson's Journal, vol. xx. p. 25.

¶ Northcote, Phil. Mag. Jan. 1855.

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The extreme saltness of this and the neighbouring lakes would appear to arise from the separation, at some remote period, of these masses of salt water from the main ocean, together with the great Caspian and Aral lakes; and the continued evaporation by constantly diminishing their volume (as has been proved by observations on the spot) has caused them ultimately to become, as they are, perfectly saturated brines: and Mr. Loftus states that there are others in the neighbourhood which have completely dried up, leaving nothing but a great bed of salt.

Sulphur Spring (Issi Su) between Dilman and Guverjin Kalah.

This spring is situated to the north-west of the Lake of Urumia; "it rises from beneath a mass of blue limestone, which emits a strong odour of sulphur. It deposits a considerable efflorescence of carbonate of soda, generally pure white, but frequently tinged with iron-yellow or red."

This water, even when received into the laboratory, contained a very considerable quantity of sulphuretted hydrogen, far more than that of the Lake of Urumia, and even more than any of the sulphuretted waters of Harrogate which I have examined; therefore at the spot its fetor must have been extreme.

Its specific gravity was 1·0142. On evaporation, a quantity of total solid residue was obtained amounting to 981·89 grains in the imperial gallon, which contained lime, magnesia in small quantity, and the alkalies; iron, a trace; also chlorine, sulphuric and carbonic acids, and silica.

I was enabled to determine the amount only of the following constituents, and give the direct results of experiments in grains in the gallon:—

Lime	6·088 grains.
Chlorine	76·140 ...
Sulphuric acid . . .	16·160 ...
Silica	7·181 ...

From these it may be approximatively stated, that the following are the saline constituents of a gallon of the water:—

Sulphate of lime	14·78 grains.
Sulphate of soda	13·24 ...
Chloride of sodium	125·47 ...
Carbonate of soda, with small quantities of the carbonates of lime and magnesia, and a trace of iron	827·22 ...
Silica	1·18 ...
Total saline constituents in the gallon	981·89 ...

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Travertin Springs.

"From the plain of Selmas northwards," says Mr. Loftus, "calcareous tufa-springs are of continual occurrence, and those of Derik are good examples of them.

"The springs are close to the village of Derik. The water rises with great force in the more northerly basin at regular intervals; but in the other irregularly, at intervals of five or seven seconds, gurgling from below, and throwing up a strong jet to the height of a foot above the surface. The temperature of the two springs is the same, viz. 96° F., indicating a common origin. As the surface of the water cools, numerous thin lamellæ of cream-coloured carbonate of lime are formed, and float about like scum. These lamellæ are sometimes 2 inches long, and about the thickness of a wafer. As the water flows onwards, cooling in its passage, it deposits the calcareous granules of carbonate of lime which become cemented together, and at length are sufficiently compact to be called travertin. These hot springs of Derik are much resorted to for every species of complaint to which the Kourd is subject." The three samples of these springs collected were the following.

No. 1. *Large Saline Spring, Derik near Selmas.*

It was collected August 18, 1852, the spring at the time having the temperature, as before mentioned, of 96° F.

This water possessed no odour of sulphuretted hydrogen, and did not contain nearly so much free carbonic acid as the preceding; but it had floating about in it granular particles of carbonate of lime, although after filtration it retained in solution only a comparatively small quantity of lime; and this might be expected; for being a hot spring, it would contain but little free carbonic acid in solution after exposure for a short time to the air, and therefore the greater part of the lime which existed in solution in the form of bicarbonate at the moment of emergence from its subterranean source, would be speedily deposited by the evaporation of the carbonic acid which held it in solution in the form of the travertin and granular carbonate of lime described by Mr. Loftus.

Its specific gravity was 1·0013, and the quantity of solid matter left on evaporation amounted to 110·027 grains in the gallon, showing it to be a far less saturated water than the two before described.

In composition this saline matter differed from either of the preceding, in that it contained but a very small quantity of chlorides, consisting chiefly of alkaline carbonates, with about 14 grains of carbonate of lime, and a minute proportion of oxide of iron, alumina, and silica.

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No. 2. *Ferruginous Saline Spring, Derik near Selmas.*

This sample was collected on the same day as that last mentioned, and had the same temperature, 96° F.

It smelt distinctly of sulphuretted hydrogen, and by exposure to the air became opake from the deposition of finely divided sulphur; there were also floating in it granular particles of carbonate of lime.

Its specific gravity was 1·0016, and it left on evaporation only 126·22 grains in the gallon of solid residue, closely resembling the saline constituents of the former spring in composition.

The quantity of iron was so small that it scarcely deserves the title of "ferruginous;" indeed in the small quantity of water sent me, it was impossible to determine its amount.

No. 3. *Chalybeate Spring at Tufa deposit.*

First Caravanserai in ravine between Khoï and Kolin.

The specimen bottle of the water of this spring was collected on the 23rd of August, 1852. It possessed the same smell of sulphuretted hydrogen as the preceding, and gave a deposit of sulphur by exposure to the air; it also contained a considerable quantity of free carbonic acid.

Its specific gravity was 1·0056, and it yielded on evaporation a quantity of solid residue, amounting to 420·113 grains in the gallon; in composition more resembling the sulphur spring of Issi Su than the saline ones of Derik, but forming, as it were, a connecting link between them.

The saline matter contained lime, magnesia, and the alkalies, (but the iron was in such small proportion that the spring scarcely deserves the name of chalybeate); also chlorine, sulphuric and carbonic acids, and silica, of which I succeeded in determining the following quantitatively:—

Lime	11·25	grains in the gallon.
Magnesia . . .	6·05	...
Chlorine . . .	102·40	...
Sulphuric acid .	3·52	...

From which it may be approximatively deduced, that the following are the saline constituents in a gallon of the water:—

Chloride of sodium . . .	168·74
Carbonate of soda	217·00
Sulphate of lime	5·98
Carbonate of lime	15·69
Carbonate of magnesia . .	12·70
Iron and silica	traces
Total	420·11 grains.

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It should be added, that the saline residues were examined for nitric, phosphoric, and boracic acids, but these bodies were not found.

The following Table may serve as a synopsis of the results obtained:—

	Specific gravity.	Total saline constituents in the gallon.	Common salt in the gallon.	Lime and magnesia salts (in solution) in the gallon.	Alkaline carbonates in the gallon.
α. Sulphuretted brine:—		grs.	grs.	grs.	grs.
Lake of Urumia	1·18812	21856·5	17254·27		
β. Alkaline saline springs:—					
Large saline spring, Derik.	1·0013	110·027	but comparatively small quantity.	14·00	The chief part.
Ferruginous saline spring Derik	1·0016	126·22	do.	.....	do.
γ. Alkaline saline brines (sulphuretted):—					
Chalybeate spring at Tufa deposit	1·0056	420·11	168·74	34·37	217·00
Sulphur spring, Issi Su...	1·0142	981·89	125·47	14·78	827·22

In conclusion, I cannot but express my regret that sufficient water had not been brought over to have enabled me to make analyses as complete as the interesting character of these waters deserved.

XXXIII. *On the Solar and Lunar Diurnal Tides of the Coasts of Ireland. By the* Rev. Samuel Haughton, *Fellow of Trinity College, Dublin.*

[Continued from p. 125.]

Section X. *Diurnal Tide at Kingstown.*

THE solar and lunar diurnal tides at Kingstown were obtained separately, as before, and found to give the following results:—

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights = 0·285 ft.
2. Maximum value of lunar tide for negative heights = 0·34 ft.
3. Maximum value of solar tide = 0·255 ft.
4. Diurnal solitidal interval = $10^h\ 26^m$.
5. Age of lunar tide = $6^d\ 17^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights = 0·27 ft.
2. Maximum value of lunar tide for negative heights = 0·40 ft.
3. Maximum value of solar tide = 0·255 ft.
4. Diurnal solitidal interval = $10^h\ 26^m$.
5. Age of lunar tide = $4^d\ 11^h$.

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Adding the first two of each of the preceding, we find—

Range of lunar tide at high water $=0{\cdot}625$ ft.
Range of lunar tide at low water $=0{\cdot}670$ ft.

Hence by equation (3),

$$\cot(m-i_m)=\frac{0{\cdot}625}{0{\cdot}670}=\cot(47^\circ);$$

or, converting the arc into time,

$$m-i_m=3^h\ 14^m;$$

but since m is the moon's hour-angle in Kingstown time, at high water, and is equal to $10^h\ 53^m$, we find

$$i_m=7^h\ 39^m.$$

By equation (4), we have

$$\text{max. value of } 2M\sin 2\mu=\sqrt{(0{\cdot}625)^2+(0{\cdot}670)^2}=0{\cdot}916 \text{ ft.};$$

from which we obtain

$$M=0{\cdot}685 \text{ ft.}$$

Also, since the maximum value of the solar tide is 0·255 ft., we find

$$\text{max. value of } 2S\sin 2\sigma=0{\cdot}510 \text{ ft.},$$

and

$$S=0{\cdot}348 \text{ ft.}$$

Combining these results, we obtain as tide constants at Kingstown,—

1. Lunitidal interval $=7^h\ 39^m$.
2. Solitidal interval $=10^h\ 26^m$.
3. Age of lunar tide
   at high water $=6^d\ 17^h$.
   at low water $=4^d\ 11^h$.
4. Lunar coefficient $=0{\cdot}685$ ft.
5. Solar coefficient $=0{\cdot}348$ ft.
6. Ratio of solar to lunar coefficient,

   or $\frac{S}{M}=0{\cdot}508$.

The theoretical tides were carefully constructed with the foregoing constants, and compared with the observed tides. The results of the comparison are contained in the following Tables:—

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Kingstown Tide, Table A.

Positive heights at high water for thirteen and a half lunations, commencing 1850, December $19^d\ 3^h$, and ending 1851, December $22^d\ 14^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·28	0·29	−0·01	8	0·33	0·32	+0·01
2	0·32	0·38	−0·06	9	0·36	0·41	−0·05
3	0·50	0·36	+0·14	10	0·37	0·39	−0·02
4	0·35	0·29	+0·06	11	0·27	0·29	−0·02
5	0·16	0·19	−0·03	12	0·20	0·19	+0·01
6	0·17	0·16	+0·01	13			
7	0·18	0·25	−0·07	14	0·28	0·31	−0·03

Mean difference = −0·005 ft.

Kingstown Tide, Table B.

Negative heights at high water for thirteen lunations, commencing 1850, December $19^d\ 3^h$, and ending 1851, December $9^d\ 8^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·53	0·40	+0·13	8	0·26	0·46	−0·20
2	0·70	0·44	+0·26	9	0·26	0·44	−0·18
3	0·40	0·40	0·00	10	0·35	0·41	−0·06
4	0·24	0·28	−0·04	11	0·33	0·29	+0·04
5	0·22	0·22	0·00	12	0·22	0·26	−0·04
6	0·36	0·30	+0·06	13			
7	0·42	0·39	+0·03				

Mean difference = 0·000 ft.

Kingstown Tide, Table C.

Positive heights at low water for thirteen lunations, commencing 1850, December $30^d\ 1^h\ 3^m$, and ending 1851, December $20^d\ 14^h\ 41^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·65	0·52	+0·13	8	0·36	0·39	−0·03
2	0·41	0·42	−0·01	9	0·27	0·29	−0·02
3	0·11	0·27	−0·16	10	0·20	0·25	−0·05
4	0·25	0·31	−0·06	11	0·28	0·30	−0·02
5	0·55	0·39	+0·16	12	0·39	0·40	−0·01
6	0·50	0·47	+0·03	13	0·52	0·51	+0·01
7	0·50	0·52	−0·02				

Mean difference = −0·004 ft.

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Kingstown Tide, Table D.

Negative heights at low water for thirteen lunations, commencing 1850, December $30^d\ 1^h\ 3^m$, and ending 1851, December $20^d\ 14^h\ 41^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·53	0·56	−0·03	8	0·45	0·45	0·00
2	0·28	0·43	−0·15	9	0 45	0·43	+0·02
3	0·42	0·39	+0·03	10	0·42	0·40	+0·02
4	0·54	0·44	+0·10	11	0·53	0·47	+0·06
5	0·45	0·53	−0·08	12			
6	0·48	0·58	−0·10	13	0·70	0·63	+0·07
7	0·56	0·57	−0·01				

Mean difference = −0·006 ft.

Kingstown Tide, Table E.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide = $6^d\ 17^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+2·7	10	+0·7	19	−0·3
2	+0·7	11	+2·2	20	−1·0
3	+0·7	12	0·0	21	−0·5
4	+0·6	13	+0·5	22	+0·7
5	+0·5	14	−1·7	23	−1·8
6	−0·5	15	−0·5	24	−0·5
7	+2·0	16	+1·0	25	−1·5
8	−2·8	17	−0·5	26	+0·3
9	−0·5	18	−0·5		

Mean difference = 0·000 days.

Kingstown Tide, Table F.

Difference of observed and calculated times of vanishing at low water, expressed in lunar days.

Age of lunar tide = $4^d\ 11^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·3	10	−0·1	19	+1·1
2	−2·3	11	−1·6	20	−0·3
3	+0·1	12	+1·2	21	+1·1
4	+2·2	13	+0·5	22	+1·1
5	+0·9	14	−0·3	23	−0·3
6	−1·7	15	+0·5	24	+0·4
7	−4·5	16	−0·7	25	+2·7
8	−1·0	17	−0·3	26	+2·2
9	−0·7	18	−0·1	27	−0·3

Mean difference = +0·004 days.

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Section XI. *Diurnal Tide at Courtown.*

On proceeding to calculate the diurnal tide constants at Courtown, I found that it was impossible to construct satisfactorily the diurnal tide at low water. The equinoctial lunar tide at low water was $\pm 0{\cdot}25$ ft. And this value was the same for the spring and autumnal equinoctial tides; but on constructing the tides from the spring equinoctial tide, I could not reconcile it with the autumnal tide, and *vice versâ*. I therefore abandoned the attempt to reconcile theory and observation with respect to the tide at low water at this station, and have only used the lunar equinoctial tide in height, which was found to be accurately the same in amount for both equinoxes.

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights $=0{\cdot}40$ ft.
2. Maximum value of lunar tide for negative heights $=0{\cdot}40$ ft.
3. Maximum value of solar tide $=0{\cdot}30$ ft.
4. Diurnal solitidal interval $=5^h\ 1^m$.
5. Age of lunar tide $=6^d\ 22^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights $=0{\cdot}25$ ft.
2. Maximum value of lunar tide for negative heights $=0{\cdot}25$ ft.
3. Maximum value of solar tide $=0{\cdot}30$ ft. ?
4. Diurnal solitidal interval $=5^h\ 1^m$. ?
5. Age of lunar tide $=3^d\ 12^h$. ?

Adding together the first two of each of the preceding results, we find—

Range of lunar tide at high water $=0{\cdot}80$ ft.
Range of lunar tide at low water $=0{\cdot}50$ ft.

Hence by equation (3),

$$\cot(m-i_m)=\frac{0{\cdot}80}{0{\cdot}50}=\cot(32^\circ);$$

or, converting the arc into time,

$$m-i_m=2^h\ 12^m;$$

but since m, the moon's hour-angle at high water expressed in Courtown time, is $7^h\ 40^m$, we obtain

$$i_m=5^h\ 28^m.$$

By equation (4), we have

$$\text{max. value of } 2M\sin 2\mu=\sqrt{(0{\cdot}80)^2+(0{\cdot}50)^2}=0{\cdot}943 \text{ ft};$$

from which we find $M=0{\cdot}719$ ft.

Also, since the mean value of the solar tide is 0·30 feet, we have

$$\text{max. value of } 2S\sin 2\sigma=0{\cdot}600 \text{ ft.},$$

and therefore $S=0{\cdot}410$ ft.

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Combining the foregoing results, we obtain for the tide constants at Courtown,—

1. Lunitidal interval $=5^h\ 28^m$.
2. Solitidal interval $=5^h\ 1^m$.
3. Age of lunar tide
   at high water $=6^d\ 22^h$.
   at low water $=3^d\ 12^h$. ?
4. Lunar coefficient $=0{\cdot}719$ ft.
5. Solar coefficient $=0{\cdot}410$ ft.
6. Ratio of solar to lunar coefficient,
   or $\frac{S}{M}=0{\cdot}570$.

The theoretical tides at high water were constructed with the foregoing constants, and compared with the observed tides. The results of this comparison are contained in the following Tables.

Courtown Tide, Table A.

Positive heights at high water for fifteen and a half lunations, commencing 1850, November $7^d\ 9^h\ 48^m$, and ending 1851, December $31^d\ 14^h\ 36^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·30	0·25	+0·05	9	0·43	0 37	+0·06
2	0·33	0·35	−0·02	10	0·52	0·50	+0·02
3	0·40	0·45	−0·05	11	0·47	0·50	−0·03
4	0·31	0·42	−0·11	12	0·34	0·38	−0·04
5	0·35	0·38	−0·03	13	0·40	0·27	+0·13
6	0·31	0·32	−0·01	14	0·27	0·23	+0·04
7	0·22	0 20	+0·02	15	0·35	0·35	0·00
8	0·27	0·29	−0·02				

Mean difference $=+0{\cdot}001$ ft.

Courtown Tide, Table B.

Negative heights at high water for fifteen and a half lunations, commencing 1850, November $7^d\ 9^h\ 48^m$, and ending 1851, December $31^d\ 14^h\ 36^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·30	0·20	+0·10	9	0·38	0·25	+0·13
2	0·17	0·22	−0·05	10	0·47	0·39	+0·08
3	0·20	0·28	−0·08	11	0·34	0·45	−0·11
4	0·45	0·45	0·00	12	0·46	0·48	−0·02
5	0·27	0·42	−0·15	13	0·50	0·33	+0·17
6	0·23	0·32	−0·09	14	0·30	0·25	+0·05
7	0·17	0·24	−0·07	15	0·25	0·25	0·00
8	0·20	0·17	+0·03	16	0·30	0·33	−0·03

Mean difference $=-0{\cdot}002$ ft.

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Courtown Tide, Table E.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide $=6^d\ 22^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	+0·31	12	+1·56	22	−1·69
2	+0·81	13	−0·94	23	+0·21
3	+0·11	14	−5·19	24	+0·41
4	−1·94	15	−1·79	25	+0·31
5	+0·81	16	−0·44	26	−2·44
6	+0·81	17	+0·41	27	+0·06
7	+1·31	18	+2·31	28	+0·81
8	−1·44	19	+1·56	29	+1·31
9	−1·99	20	+1·56	30	+0·81
10	+0·31	21	+0·81	31	+0·81
11	+0·31				
Mean difference $=-0·004$ days.					

The agreement between theory and observation shown in the preceding Tables is very satisfactory; and were it not for the difficulties presented in the low water observations, we would consider the tides at Courtown well represented by the theoretical tides. This discrepancy between theory and observation at Courtown is probably connected with the peculiarities of the Courtown tides, which have been brought to light by the Astronomer Royal in his discussion of the semidiurnal tide at this station.

Section XII. *Diurnal Tide at Dunmore East.*

Having obtained the values of the diurnal tide for each day at Dunmore, I calculated the constants from them in the usual manner, and found,—

I. *Diurnal tide at high water.*

1. Maximum value of lunar tide for positive heights $=0·22$ ft.
2. Maximum value of lunar tide for negative heights $=0·21$ ft.
3. Maximum value of solar tide $=0·14$ ft.
4. Diurnal solitidal interval $=5^h\ 15^m$.
5. Age of lunar tide $=5^d\ 19^h$.

II. *Diurnal tide at low water.*

1. Maximum value of lunar tide for positive heights $=0·18$ ft.
2. Maximum value of lunar tide for negative heights $=0·19$ ft.
3. Maximum value of solar tide $=0·14$ ft.
4. Diurnal solitidal interval $=5^h\ 15^m$.
5. Age of lunar tide $=5^d\ 14^h$.

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Adding together the first two of each of the preceding, we find—

Range of lunar tide at high water $=0{\cdot}43$ ft.
Range of lunar tide at low water $=0{\cdot}37$ ft.

Hence by equation (3),

$$\cot(m-i_m)=\frac{0{\cdot}43}{0{\cdot}37}=\cot(40^\circ\ 43');$$

or, converting the arc into time,

$$m-i_m=2^h\ 48^m;$$

but since m, the moon's hour-angle at high water expressed in Dunmore time, is $4^h\ 36^m$, we obtain

$$i_m=1^h\ 48^m.$$

By equation (4), we have

$$\text{max. value of } 2M\sin 2\mu=\sqrt{(0{\cdot}43)^2+(0{\cdot}37)^2}=0{\cdot}567 \text{ ft.};$$

from which we obtain

$$M=0{\cdot}441 \text{ ft.}$$

Also, since the mean value of the solar tide is 0·14 feet, we have by equation (5),

$$\text{max. value of } 2S\sin 2\sigma=0{\cdot}28 \text{ ft.},$$

and

$$S=0{\cdot}192 \text{ ft.}$$

Combining the foregoing results, we obtain for the tide constants at Dunmore,—

1. Lunitidal interval $=1^h\ 48^m$.
2. Solitidal interval $=5^h\ 15^m$.
3. Age of lunar tide
   at high water $=5^d\ 19^h$.
   at low water $=5^d\ 14^h$.
4. Lunar coefficient $=0{\cdot}441$ ft.
5. Solar coefficient $=0{\cdot}192$ ft.
6. Ratio of solar to lunar coefficient,

   or $\frac{S}{M}=0{\cdot}436$.

The theoretical tides were constructed with the foregoing tide constants, and compared with the observed tides, with the following results.

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Dunmore Tide, Table A.

Positive heights at high water for seventeen lunations, commencing 1850, September 11^d 21^h 30^m, and ending 1851, December 20^d 15^h 42^m.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·17	0·16	+0·01	10	0·31	0·24	+0·07
2	0·30	0·17	+0·13	11	0·28	0·30	−0·02
3	0·20	0·22	−0·02	12	0·30	0·32	−0·02
4	0·35	0·26	+0·09	13	0·34	0·29	+0·05
5	0·26	0·29	−0·03	14	0·12	0·22	−0·10
6	0·13	0·27	−0·14	15	0·25	0·20	+0·05
7	0·17	0·24	−0·07	16	0·28	0·24	+0·04
8	0·17	0·16	+0·01	17	0·21	0·29	−0·08
9	0·19	0·18	+0·01				

Mean difference = −0·001 ft.

Dunmore Tide, Table B.

Negative heights at high water for seventeen lunations, commencing 1850, September 11^d 21^h 30^m, and ending 1851, December 20^d 15^h 42^m.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·21	0·21	0·00	10	0·27	0 24	+0·03
2	0·27	0·16	+0·11	11	0·28	0·28	0·00
3	0 23	0·19	+0·04	12	0·30	0·31	−0·01
4	0·27	0·28	−0 01	13	0·18	0·28	−0·10
5	0·28	0·29	−0·01	14	0·29	0·28	+0·01
6	0·08	0·27	−0·19	15	0·29	0·16	+0·13
7	0·11	0·27	−0·16	16	0·26	0·22	+0·04
8	0·20	0·19	+0·01	17	0·32	0·28	+0·04
9	0·24	0·17	+0·07				

Mean difference = 0·000 ft.

Dunmore Tide, Table C.

Positive heights at low water for seventeen lunations, commencing 1850, September 13^d 17^h 0^m, and ending 1851, December 22^d 22^h 30^m.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·11	0·13	−0·02	10	0·06	0·07	−0·01
2	0·15	0·14	+0·01	11	0·08	0·09	−0·01
3	0·05	0·06	−0·01	12	0·13	0·13	0·00
4	0·28	0·07	+0·21	13	0·18	0·19	−0·01
5	0·18	0·14	+0 04	14	0·18	0·15	+0·03
6	0·17	0·16	+0·01	15	0·14	0·11	+0·03
7	0·20	0·21	−0·01	16	0·13	0·11	+0·02
8	0·08	0·15	−0·07	17	0·14	0·14	0·00
9	0·10	0·11	−0·01				

Mean difference = +0·001 ft.

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Dunmore Tide, Table D.

Negative heights at low water for seventeen lunations, commencing 1850, September $13^d\ 17^h\ 0^m$, and ending 1851, December $22^d\ 22^h\ 30^m$.

No.	Observed.	Calculated.	Difference.	No.	Observed.	Calculated.	Difference.
	ft.	ft.	ft.		ft.	ft.	ft.
1	0·23	0·22	+0·01	10	0·15	0·12	+0·03
2	0·18	0·14	+0·04	11	0·09	0·11	−0·02
3	0·23	0·13	+0·10	12	0·20	0·12	+0·08
4	0·23	0·12	+0·11	13	0·10	0·18	−0·08
5	0·13	0·12	+0·01	14	0·14	0·22	−0·08
6	0·18	0·19	−0·01	15	0·15	0·16	−0·01
7	0·18	0·20	−0·02	16	0·15	0·13	+0·02
8	0·18	0·20	−0·02	17	0·20	0·13	+0·07
9	0·11	0·13	−0·02				
Mean difference = +0·006 ft.							

Dunmore Tide, Table E.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide = $5^d\ 19^h$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	−2·65	13	−2·40	25	−1·15
2	−1·56	14	−0·90	26	+1·35
3	+1·60	15	+0·10	27	−0·90
4	+0·30	16	−1·56	28	−0·40
5	−0·90	17	−0·90	29	−0·90
6	+1·85	18	−2·90	30	+0·80
7	+0·60	19	+0·10	31	0·00
8	+1·60	20	−1·90	32	+2·60
9	+1·10	21	−1·90	33	−0·20
10	+1·10	22	+0·50	34	+2·10
11	+4·10	23	−1·15	35	−0·50
12	+1·60	24	+1·35		
Mean difference = 0·000 days.					

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Dunmore Tide, Table F.

Difference of observed and calculated times of vanishing at high water, expressed in lunar days.

Age of lunar tide $=5^{d}\ 14^{h}$.

No.	Difference.	No.	Difference.	No.	Difference.
	days.		days.		days.
1	−0·10	13	+0·20	25	+1·40
2	−1·20	14	−0·60	26	+1·40
3	−0·60	15	+1·70	27	+1·10
4	−0·60	16	+1·90	28	+1·40
5	−2·70	17	−0·60	29	−0·60
6	−2·00	18	−2·60	30	−3·10
7	−4·60	19	+0·40	31	+0·20
8	−0·60	20	−0·60	32	−1·00
9	+1·90	21	−3·10	33	−0·60
10	+1·40	22	−0·60	34	−0·10
11	+1·40	23	+0·40	35	+3·40
12	+2·80	24	+4·90		

Mean difference = 0·000 days.

The agreement between the calculated and observed tides shown in the preceding Tables is excellent; and since the tide itself is small, it proves the remarkable care with which the observations were taken at this station.

In the next section, which will conclude this abstract, I intend to give some general deductions from the preceding facts, and to compare the results of observation with theory.

[To be continued.]

XXXIV. *On the Galactite of* Haidinger, *with Analyses of Scotch Natrolites. By* Dr. Heddle*.

IN seeking out specimens of "Galactite" for analysis, my first difficulty lay in ascertaining if they were the species to which Haidinger had given this name; and finding that Mr. Rose's Galactites came from a totally different part of the country from that which afforded my own specimens, I bethought myself of writing to Mr. Greg, the catalogue of whose collection was compiled by Haidinger himself: the result was that Mr. Greg sent me Galactites from two localities, neither of which I had imagined to be the true one.

As Mr. Greg's localities must be correct, I give them the preference in my analyses. The first was Glenfarg in Fifeshire; the specimen sent was white, though not very milky; its ana-

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lysis (on 12·5 grs.) afforded,—

Silica	48·24
Alumina	27·00
Lime	·82
Soda	14·82
Water	9·24
	100·12

As the Glenfarg mineral, however, passes from white to red, by far its most frequent colour, I also submitted a deep red specimen to analysis, to ascertain what the constitutional difference might be; I obtained (on 25 grs.*),—

Silica	47·84
Alumina	27·112
Lime	4·312
Soda	11·304
Water	10·24
	100·808

Here a considerable quantity of the soda is replaced by lime; the colouring matter I could not ascertain.

Haidinger's second locality is the Campsie Hills, the exact spot I do not know; so convinced was I on receiving it that this was a specimen of decomposed Laumontite, that it lay for more than a year unnoticed; on analysis it afforded,—

Silica	47·324
Alumina	27·36
Lime	2·63
Soda	13·354
Water	10·392
	101·060

This is evidently the same substance.

The mineral which is generally understood by the Edinburgh mineralogists to be the Galactite of Haidinger, is found at Bishoptown, at the locality whence Greenockite was obtained; it occurs here of a milky, and also of a delicate pinkish-cream colour. I analysed both:—

	The white.	The pink.
Silica . . .	47·60	47·76
Alumina . .	26·60	27·20
Lime . . .	·16	·93
Soda . . .	15·86	14·28
Water . . .	9·56	9·56
	99·78	99·72

* When not otherwise expressed, my analyses are on 25 grs.

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A radiated mineral of a still more decided pink tinge, which is found at Glenarbuck and the Long Craig in Dumbartonshire, is also called Galactite. The fibres of all the specimens that I have seen run so much into the Kilpatrick or zeolitic quartz, that I have not been able to free any specimen sufficiently from this matrix to rely upon the correctness of a quantitative analysis; by a qualitative examination, however, I have ascertained that this is the same substance as the above; lime here also being present in small quantity.

These are, I believe, all the localities of Galactite, and all are in composition merely Natrolite (the calculated per-centages of which, for the sake of comparison, are appended in a foot-note*); a small, generally a trifling, proportion of lime in each replaces a portion of the soda, the full amount of which in Natrolite is 16·2 per cent.; this small portion of lime it is which gives to these Natrolites their whiteness and opacity, and doubtless prevents their assuming the definite crystalline form which the pure mineral under favourable circumstances adopts.

Natrolite, though not always recognized as such, occurs in Scotland at several other localities; at Bowling quarry and at Cochna near old Kilpatrick (as also, I am informed, at Bishoptown), it assumes an appearance very different from its usual aspect. It is here associated with Laumontite, the sheafy variety, and dark green talc, the matrix being highly magnesian in its immediate vicinity. It occurs in spheres imbedded in the rock; these are white at the centre, but of a fine green at the circumference, apparently from the radiating crystals penetrating the matrix. The specimens from this locality have been sold as Stellite, which (see Phil. Mag. for April 1855) has been shown to be Pectolite, and Dr. R. D. Thomson gave it the latter name to Mr. Greg. Its analysis afforded,—

Silica	48·033
Alumina . . .	25·261
Oxide of iron . .	·865
Lime	2·313
Magnesia . . .	·403
Soda	13·975
Water	9·723
	100·573

A single colourless specimen, which I myself obtained at Bow-

* Silica . . . . . 47·4
Alumina . . . . . 26·9
Soda . . . . . 16·2
Water . . . . . 9·5

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ling, contained neither magnesia nor iron, which are doubtless derived from the matrix.

My specimens from the next locality, Dumbarton Moor, so much resemble the Glenfarg specimens, that it is impossible to distinguish between them. The specimens from this locality are perhaps the finest that Scotland affords. My analysis was made on a mixed red and white specimen:—

Silica	46·96
Alumina	26·908
Lime	3·76
Soda	12·83
Water	9·50
	99·958

This species is also said to occur at the Bin above Burntisland in Fife, and near North Berwick: I have not seen specimens from these places, the above being all the Scotch localities I know. Glenfarg is, as far as I am aware, the only one of these where Natrolite occurs distinctly crystallized; the form is *o m* of Brooke and Miller. These crystals contain merely a trace of lime, but it is singular that elsewhere in Scotland it should always contain that base; I should, however, mention that Dr. Scott, who analysed the Bishoptown variety (see Edinb. New Phil. Journ. for Oct. 1852), found no lime. I have, however, examined certainly a dozen specimens from this locality, and invariably detected its presence.

In this species, as in many others of the Zeolites, the quantity of water varies considerably, even in specimens from the *same* locality—sometimes as much as 1 per cent.

XXXV. *Note on the Theory of Logarithms.* By A. Cayley, *Esq.**

An imaginary quantity $x+yi$ may always be expressed in the form

$$x+yi=r(\cos\theta+i\sin\theta)=re^{\theta i},$$

where r is positive, and θ is included between the limits $-\pi$ and $+\pi$. We have, in fact,

$$r=\sqrt{x^2+y^2};$$

and when x is positive,

$$\theta=\tan^{-1}\frac{y}{x};$$

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but when x is negative,

$$\theta = \tan^{-1}\frac{y}{x} \pm \pi;$$

where $\tan^{-1}$ denotes an arc between the limits $-\frac{\pi}{2}$, $+\frac{\pi}{2}$, and where the upper or under sign is to be employed according as y is positive or negative. I use for convenience the mark $\equiv$ to denote identity of sign; we may then write

$$\theta = \tan^{-1}\frac{y}{x} + \epsilon\pi,$$

where

$$x \equiv +,\ \epsilon = 0,$$

$$x \equiv -,\ \epsilon = \pm 1 \equiv y.$$

It should be remarked that θ has a unique value except in the single case $x \equiv -$, $y = 0$, where θ is indeterminately $\pm\pi$. We have, in fact, $\theta = +\pi$ or $\theta = -\pi$ according as x is considered as the limit of $x+yi$, $y \equiv +$, or of $x+yi$, $y \equiv -$. It is natural to write

$$\log(x+yi) = \log r + \theta i,$$

or what is the same thing,

$$\log(x+yi) = \log\sqrt{x^2+y^2} + \left(\tan^{-1}\frac{y}{x} + \epsilon\pi\right)i;$$

and I take this equation as the definition of the logarithm of an imaginary quantity. The question then arises, to find the value of the expression

$$\log(x+yi) + \log(x'+y'i) - \log(x+yi)(x'+y'i).$$

The preceding definition is, in fact, in the case of x positive, that given by M. Cauchy in the *Exercices de Mathématique*, vol. i.; and he has there shown that x, x', $xx'-yy'$ being all of them positive, the above-mentioned expression reduces itself to zero. The general definition is that given in my *Mémoire sur quelques Formules du Calcul Intégral*, Liouville, vol. xii. p. 231; but I was wrong in asserting that the expression always reduced itself to zero. We have, in fact, in general

$$\tan^{-1}\alpha + \tan^{-1}\beta = \tan^{-1}\frac{\alpha+\beta}{1-\alpha\beta},$$

when $1-\alpha\beta$ is positive; but when $1-\alpha\beta$ is negative (which implies that α, β have the same sign), then

$$\tan^{-1}\alpha + \tan^{-1}\beta = \tan^{-1}\frac{\alpha+\beta}{1-\alpha\beta} \pm \pi,$$

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where the upper or under sign is to be employed according as α and β are positive or negative; or what is the same thing,

$$\tan^{-1}\alpha+\tan^{-1}\beta=\tan^{-1}\frac{\alpha+\beta}{1-\alpha\beta}+\epsilon\pi,$$

where

$$1-\alpha\beta\equiv +,\quad \epsilon=0,$$
$$1-\alpha\beta\equiv -,\quad \epsilon=\pm 1\equiv\alpha+\beta\equiv\alpha\equiv\beta.$$

This being premised, then writing

$$\log(x+yi)=\log\sqrt{x^2+y^2}+\left(\tan^{-1}\frac{y}{x}+\epsilon\pi\right)i$$

$$\log(x'+y'i)=\log\sqrt{x'^2+y'^2}+\left(\tan^{-1}\frac{y'}{x'}+\epsilon'\pi\right)i$$

$$\log(x+yi)(x'+y'i)=\log\left[(xx'-yy')+(xy'+yx')i\right]$$
$$=\log\sqrt{x^2+y^2}\sqrt{x'^2+y'^2}+\left(\tan^{-1}\frac{xy'+x'y}{xx'-yy'}+\epsilon''\pi\right)i$$

$$\tan^{-1}\frac{y}{x}+\tan^{-1}\frac{y'}{x'}=\tan^{-1}\frac{xy'+x'y}{xx'-yy'}+\epsilon'''\pi,$$

we find

$$\log(x+yi)+\log(x'+y'i)-\log(x+yi)(x'+y'i)=(\epsilon+\epsilon'-\epsilon''+\epsilon''')\pi i.$$

Hence, considering the different cases,—

I. $x\equiv +,\quad x'\equiv +,\quad xx'-yy'\equiv +$

$\epsilon=0$

$\epsilon'=0$

$\epsilon''=0$

$\epsilon'''=0,$

and therefore $\epsilon+\epsilon'-\epsilon''+\epsilon'''=0$.

II. $x\equiv +,\quad x'\equiv +,\quad xx'-yy'\equiv -$

$\epsilon=0$

$\epsilon'=0$

$\epsilon''=\pm 1\equiv(xy'+x'y)\equiv\left(\frac{y}{x}+\frac{y'}{x'}\right)$

$\epsilon'''=\pm 1\qquad\qquad\equiv\left(\frac{y}{x}+\frac{y'}{x'}\right),$

and therefore $\epsilon+\epsilon'-\epsilon''+\epsilon'''=0$.

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III. $x \equiv +,\quad x' \equiv -,\quad xx' - yy' \equiv +$

$$\epsilon = 0$$
$$\epsilon' = \pm 1 \equiv y' \equiv -\frac{y'}{x'} \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right)$$
$$\epsilon'' = 0$$
$$\epsilon''' = \pm \qquad \equiv \left(\frac{y}{x} + \frac{y'}{x'}\right),$$

and therefore $\epsilon + \epsilon' - \epsilon'' + \epsilon''' = 0$.

IV. $x \equiv +,\quad x' \equiv -,\quad xx' - yy' \equiv -$

$$\epsilon = 0$$
$$\epsilon' = \pm 1 \equiv y' \equiv -\frac{y'}{x'}$$
$$\epsilon'' = \pm 1 \equiv xy' + x'y \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right)$$
$$\epsilon''' = 0,$$

and therefore $\epsilon + \epsilon' - \epsilon'' + \epsilon''' = 0$ if $\frac{y'}{x'} \equiv \left(\frac{y}{x} + \frac{y'}{x'}\right)$,

but

$$\epsilon + \epsilon' - \epsilon'' + \epsilon''' = \pm 2 \equiv \left(\frac{y}{x} + \frac{y'}{x'}\right) \text{ if } \frac{y'}{x'} \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right).$$

V. $x \equiv -,\quad x' \equiv +,\quad xx' - yy' \equiv +$

$$\epsilon = \pm 1 \equiv y \equiv -\frac{y}{x} \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right)$$
$$\epsilon' = 0$$
$$\epsilon'' = 0$$
$$\epsilon''' = \pm 1 \qquad \equiv \left(\frac{y}{x} + \frac{y'}{x'}\right),$$

and therefore $\epsilon + \epsilon' - \epsilon'' + \epsilon''' = 0$.

VI. $x \equiv -,\quad x' \equiv +,\quad xx' - yy' \equiv -$

$$\epsilon = \pm 1 \equiv y \equiv -\frac{y}{x}$$
$$\epsilon' = 0$$
$$\epsilon'' = \pm 1 \equiv (xy' + x'y) \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right)$$
$$\epsilon''' = 0,$$

and therefore $\epsilon + \epsilon' - \epsilon'' + \epsilon''' = 0$ if $\frac{y}{x} \equiv \left(\frac{y}{x} + \frac{y'}{x'}\right)$,

but $\epsilon + \epsilon' - \epsilon'' + \epsilon''' = \pm 2 \equiv \frac{y}{x} + \frac{y'}{x'}$ if $\frac{y}{x} \equiv -\left(\frac{y}{x} + \frac{y'}{x'}\right)$.

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VII. $x \equiv -,\quad x' \equiv -,\quad xx'-yy' \equiv +$

$$\epsilon = \pm 1 \equiv y \equiv -\frac{y}{x}$$

$$\epsilon' = \pm 1 \equiv y' \equiv -\frac{y'}{x'}$$

$$\epsilon'' = 0$$

$$\epsilon''' = 0;$$

and therefore $\epsilon+\epsilon'-\epsilon''+\epsilon'''=0$ if $\frac{y}{x} \equiv -\frac{y'}{x'}$,

but $\epsilon+\epsilon'-\epsilon''+\epsilon''' = \pm 2 \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$ if $\frac{y}{x} \equiv \frac{y'}{x'}$.

VIII. $x \equiv -,\quad x' \equiv -,\quad xx'-yy' \equiv -$

$$\epsilon = \pm 1 \equiv y \equiv -\frac{y}{x} \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$$

$$\epsilon' = \pm 1 \equiv y' \equiv -\frac{y'}{x'} \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$$

$$\epsilon'' = \pm 1 \equiv (xy'+x'y) \equiv \left(\frac{y}{x}+\frac{y'}{x'}\right)$$

$$\epsilon''' = \pm 1 \equiv \left(\frac{y}{x}+\frac{y'}{x'}\right),$$

and therefore $\epsilon+\epsilon'-\epsilon''+\epsilon''' = \pm 2 \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$.

Hence writing

$$\log(x+yi)+\log(x'+y'i)-\log(x+yi)(x'+y'i) = \mathrm{E}\pi i,$$

we have $\mathrm{E}=0$, except in the following cases, viz.—

1. (See IV.) $x \equiv +,\ x' \equiv -,\ xx'-yy' \equiv -,\ \frac{y'}{x'} \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$,

where $\mathrm{E} = \pm 2 \equiv \left(\frac{y}{x}+\frac{y'}{x'}\right)$.

2. (See VI.) $x \equiv -,\ x' \equiv +,\ xx'-yy' \equiv -,\ \frac{y}{x} \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$,

where $\mathrm{E} = \pm 2 \equiv \left(\frac{y}{x}+\frac{y'}{x'}\right)$.

3. (See VII.) $x \equiv -,\ x' \equiv -,\ xx'-yy' \equiv +,\ \frac{y}{x} \equiv \frac{y'}{x'}$,

where $\mathrm{E} = \pm 2 \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$.

4. (See VIII.) $x \equiv -,\quad x' \equiv -,\quad xx'-yy' \equiv -$,

where $\mathrm{E} = \pm 2 \equiv -\left(\frac{y}{x}+\frac{y'}{x'}\right)$.

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It thus appears that when the real parts $x, x', xx'-yy'$ are all three of them positive, or any two of them positive and the third negative, E is equal to zero, or the logarithm of the product is equal to the sum of the logarithms of the factors; but that if the real parts are one of them positive and the other two of them negative, then if a certain relation between the real and imaginary parts is satisfied, but not otherwise, the property holds; and if the real parts are all three of them negative, the property does not hold in any case.

The preceding results do not apply to the case where any one of the arguments $x+yi$, $x'+y'i$, $(x+yi)(x'+y'i)$ is real and negative, for no definition applicable to such case has been given of a logarithm. If, however, we assume as a definition that the logarithm of a negative real quantity is equal to the logarithm of the corresponding positive quantity, then it is easy to obtain $x \equiv -$, $y=0$,

$$\log x + \log(x'+y'i) - \log x(x'+y'i) = \epsilon\pi i, \quad \epsilon = \pm 1 \equiv y';$$

an equation which is, in fact, equivalent to

$$\log(x'+y'i) - \log[-(x'+y'i)] = \epsilon\pi i, \quad \epsilon = \pm 1 \equiv y'.$$

And $xy'+x'y=0$, $xx'-yy' \equiv -$, which implies $y \equiv y'$, then

$$\log(x+yi) + \log(x'+y'i) - \log(x+yi)(x'+y'i) = \pi i,$$
$$\epsilon = \pm 1 \equiv y \text{ or } y';$$

an equation which is in fact equivalent to

$$\log(x+yi) + \log(-x+yi) - \log(x^2+y^2) = \epsilon\pi i, \ \epsilon = \pm 1 \equiv y.$$

The case where both of the arguments $x+yi$, $x'+y'i$ are real and negative, *i. e.* $x \equiv -$, $y=0$, $x' \equiv -$, $y'=0$ gives of course $\log x + \log x' - \log xx' = 0$, the logarithms of the negative real quantities x, x' being by the definition the same as the logarithms of the corresponding positive quantities. It should, however, be remarked that the definition $(x \equiv -) \log x = \log(-x)$ not only gives for $\log x$ a different value from that which would be obtained from the general definition of a logarithm, by considering $\log x$ as the limit of $\log(x+yi)$, $y \equiv +$, or of $\log(x+yi)$, $y \equiv -$, but gives also a value, which, for the particular case in question, contradicts the fundamental equation $e^{\log x} = x$. It is therefore, I think, better not to establish any definition for the logarithm of a negative real quantity x, but to say that such logarithm is absolutely indeterminate and indeterminable, except in the case where, from the nature of the question, x is considered as the limit of $x+yi$, y positive, or of $x+yi$, y negative.

2 Stone Buildings,
March 15, 1856.

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XXXVI. *On the Dynamical Theory of Heat.*—Part VI. *Thermo-electric Currents*.* *By* WILLIAM THOMSON, *M.A., Professor of Natural Philosophy in the University of Glasgow.*

[Continued from p. 225.]

§§ 112–124. *General Equations of Thermo-electric Currents in non-crystalline Linear Conductors.*

112. THE only reversible thermal effect of electric currents which experiment has yet demonstrated, is that which Peltier has discovered in the passage of electricity from one metal to another. Besides this, we may conceive that in one homogeneous metal formed into a conductor of varying section, different thermal effects may be produced by a current in any part, according as it passes in the direction in which the section increases, or in the contrary direction; and with greater probability we may suppose, that a current in a conductor of one metal unequally heated may produce different thermal effects, according as it passes from hot to cold, or from cold to hot. But Magnus has shown by careful experiments, that no application of heat can sustain a current in a circuit of one homogeneous non-crystalline metal, however varying in section; and from this it is easy to conclude, by equations (7) and (9), that there can be no reversible thermal effect due to the passage of a current between parts of a homogeneous metallic conductor having different sections. Now it is clear that no circumstances, except those which have just been mentioned, can possibly give rise to different thermal effects in any part of a

* Foot note on the third paragraph of § 109—"And (2), That JB, which cannot vanish in any case, is the absolute numerical measure of the galvanic resistance of the principal conductor itself"—contained in previous article (p. 224).

Principle of Mechanical Action on Electromotive Forces and Galvanic Resistances.

This conclusion was first given by Joule in his first paper, which was communicated to the Royal Society, December 17, 1840, "On the Production of Heat by Voltaic Electricity" (see 'Proceedings' of that date). The paper was published in the Philosophical Magazine, vol. xix. p. 260. See also "On the Calorific Effects of Magneto-electricity, and the Mechanical Value of Heat," by the same author (Phil. Mag. vol. xxiii. 1843), where the principles of mechanical action in the electric generation of heat are more fully developed.

The conclusion stated in the text was also given by Helmholtz in his *Erhaltung der Kraft*, Berlin, 1847 (translated in Taylor's New Scientific Memoirs). It was given by the author of the present paper with various numerical applications regarding the electromotive forces of electro-chemical arrangements and the resistances of metallic conductors in absolute units, in two papers in the Philosophical Magazine, December 1851, "On the Mechanical Theory of Electrolysis," and "On the Applications of the Principle of Mechanical Effect," &c.

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linear conductor of the same or of different metals, uniformly or non-uniformly heated, provided none of them be crystalline; and we have therefore at present nothing in the sum $\Sigma\alpha_t$, besides the terms depending on the passage of electricity from one metal to another, which certainly exist, and terms which may possibly be discovered, depending on its passage from hot to cold, or from cold to hot, in the same metal.

113. Let the principal conductor consist of n different metals; in all $n+1$ parts, of which the first and last are of the same metal, and have their terminal portions (which we have called the electrodes E and E') at the same temperature T_0. Let T_1, T_2, T_3, &c. denote the temperatures of the different junctions in order, and let Π_1, Π_2, Π_3, &c. denote the amounts (positive or negative) of heat absorbed at them respectively by a positive current of unit strength during the unit of time. Let $\gamma\sigma_1 dt$, $\gamma\sigma_2 dt$, $\gamma\sigma_3 dt$, &c. denote the quantities of heat evolved in each of the different metals in the unit of time by a current of infinitely small strength, γ, passing from a locality at temperature $t+dt$ to a locality at temperature t. Without hypothesis, but by an obvious analogy, we may call the elements σ_1, σ_2, &c. the *specific heats of electricity in the different metals*, since they express the quantities of heat absorbed or evolved by the unit of current electricity in passing from cold to hot, or from hot to cold, between localities differing by a degree of temperature in each metal respectively. It is easily shown (as will be seen by the treatment of the subject to follow immediately) that if the values of σ_1, σ_2, &c. depend either on the *section* of the conductor, or on the rate of variation of temperature along it, or on any other variable differing in different parts of the conductor, except the temperature, a current might be maintained by the application of heat to a homogeneous metallic conductor. We may therefore at once assume them to be, if not invariable, absolute functions of the temperature. From this it follows, that if ϕt denote any function of t, the value of the sum $\int\phi t\sigma dt$ for any conducting arc of homogeneous metal depends only on the temperatures of its extremities, and therefore the parts of the sums $\Sigma\alpha_t$ and $\frac{\Sigma\alpha_t}{t}$, corresponding to the successive metals in the principal conductor, are respectively

$$-\int_{T_1}^{T_0}\sigma_1 dt, \quad -\int_{T_2}^{T_1}\sigma_2 dt, \ldots . -\int_{T_n}^{T_{n-1}}\sigma_n dt, \quad -\int_{T_0}^{T_n}\sigma_1 dt,$$

and

$$-\int_{T_1}^{T_0}\frac{\sigma_1}{t}dt, \quad -\int_{T_2}^{T_1}\frac{\sigma_2}{t}dt \ldots . -\int_{n}^{T_{n-1}}\frac{\sigma_n}{t}dt, \quad -\int_{T_0}^{T_n}\frac{\sigma_1}{t}dt.$$

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Hence the general equations (7) and (9) become

$$F=J\left\{\Pi_1+\Pi_2+\ldots.+\Pi_n-\int_{T_1}^{T_0}\sigma_1 dt-\int_{T_2}^{T_1}\sigma_2 dt-\ldots.\right.$$
$$\left.-\int_{T_n}^{T_{n-1}}\sigma_n dt-\int_{T_0}^{T_n}\sigma_1 dt\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

$$\frac{\Pi}{T_1}+\frac{\Pi_2}{T_2}+\ldots+\frac{\Pi_n}{T_n}-\int_{T_1}^{T_0}\frac{\sigma_1}{t}dt-\int_{T_2}^{T_1}\frac{\sigma}{t}dt-\ldots.-\int_{T_n}^{T_{n-1}}\frac{\sigma_n}{t}dt$$
$$-\int_{T_0}^{T_n}\frac{\sigma_1}{t}dt=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

which are the fundamental equations of thermo-electricity in non-crystalline conductors. In these, along with the equation

$$\gamma=\frac{P+F}{JB} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

which shows the strength of the current actually sustained in the conductor when an independent electromotive force, P, is applied between the principal electrodes E, E′, we have a full expression of the most general circumstances of thermo-electric currents in linear conductors of non-crystalline metals.

114. The special qualities of the metals of a thermo-electric circuit must be investigated experimentally before we can fix the values of Π_1, Π_2, &c., and σ_1, σ_2, &c. for any particular case. The relation between these quantities expressed in the general equation (11) having, as we have seen, a very high degree of probability, not merely as an approximate law, but as an essential truth, may be used as a guide, but must be held provisionally until we have sufficient experimental evidence in its favour. The first fundamental equation (10) admits of no doubt whatever as to its universal application, and we shall see (§ 123*) that it leads to most remarkable conclusions from known experimental facts.

The general principles are most conveniently applied by restricting the number of metals referred to in the general equations to two; a case which we accordingly proceed to consider.

115. Let the principal conductor consist of two metals, one constituting the middle, and the other the two terminal portions. Let the junctions of these portions next the terminals E, E′ be denoted by A, A′ respectively, and let their temperatures be T, T′. Let also $\Pi(T)$, $-\Pi(T')$ be the quantities of heat absorbed at them per second by a current of unit strength. We should

* See Proceedings of the Royal Society, Phil. Mag. July 1854, p. 63.

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have

$$\Pi(T)=\Pi(T'),$$

if the temperatures were equal, since the Peltier phænomenon consists, as we have seen, of equal quantities of heat evolved or absorbed, according to the direction of a current crossing the junction of two different metals; and if these quantities be not actually equal, we may consider them as particular values of a function Π of the temperature, which depends on the particular relative thermo-electric quality of the two metals. Accordingly, the preceding notation is reduced to $n=2$, $T_1=T$, $T_2=T'$, $\Pi_1=\Pi(T)$, $\Pi_2=-\Pi(T')$; and we have

$$\int_{T_1}^{T_0}\sigma_1 dt+\int_{T_2}^{T_1}\sigma_2 dt+\int_{T_0}^{T_2}\sigma_1 dt=\int_{T}^{T'}(\sigma_1-\sigma_2)dt,$$

and similarly for the integral involving $\frac{1}{t}$. Hence the general equations become

$$F=J\left\{\Pi(T)-\Pi(T')+\int_{T'}^{T}(\sigma_1-\sigma_2)dt\right\} \quad . \quad . \quad (13)$$

$$\frac{\Pi(T)}{T}-\frac{\Pi(T')}{T'}+\int_{T'}^{T}\frac{\sigma_1-\sigma_2}{t}dt=0 \quad . \quad . \quad . \quad . \quad (14).$$

If in the latter equation we substitute t for T, and differentiate with reference to this variable, we have as an equivalent equation,

$$\frac{d\left(\frac{\Pi}{t}\right)}{dt}+\frac{\sigma_1-\sigma_2}{t}=0 \quad . \quad . \quad . \quad . \quad (15),$$

or

$$\sigma_1-\sigma_2=\frac{\Pi}{t}-\frac{d\Pi}{dt} \quad . \quad . \quad . \quad . \quad . \quad (16).$$

This last equation leads to a remarkably simple expression for the electromotive force of a thermo-electric pair, solely in the terms of the Peltier evolution of heat at any temperature intermediate between the temperatures of its junctions; for we have only to eliminate by means of it $(\sigma_1-\sigma_2)$ from (13), to find

$$F=J\int_{T'}^{T}\frac{\Pi}{t}dt \quad . \quad . \quad . \quad . \quad . \quad . \quad (17).$$

116. Let us first apply these equations to the case of a thermo-electric pair, with the two junctions kept at temperatures differing by an infinitely small amount τ. In this case we have

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$$\Pi(T) - \Pi(T') = \frac{d\Pi}{dt}\tau,$$

$$\int_{T'}^{T} (\sigma_1 - \sigma_2)dt = (\sigma_1 - \sigma_2)\tau;$$

and equation (13) becomes

$$F = J\left\{\frac{d\Pi}{dt} + \sigma_1 - \sigma_2\right\}\tau \quad . \quad . \quad . \quad (18).$$

If we make use of (16) in this, we have

$$F = J\frac{\Pi}{t}\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

The first of these expressions for the electromotive force involves no hypothesis, but only the general principle of equivalence of heat and work. Its agreement with any experimental results is only to be looked on as a verification of the accuracy of the experiments, and can add nothing to the certainty of the part of the theory from which it is deduced. On the other hand, it would be extremely important to test the second expression (19) by direct experiment, and so confirm or correct the only doubtful part of the theory. The way to do so would be to determine in absolute measure the electromotive force, F, due to a small difference of temperature, τ, in any thermo-electric pair, and to determine, in known thermal units, the amount of the Peltier effect at a junction of the two metals with a current of strength measured in electro-dynamic units, as we should then, by these determinations, be able to evaluate from direct experiments the values of the two members separately which appear equated in (19). As yet no observations have been made which lead directly or indirectly to the evaluation of the second member of (19) in any case, but I hope before long to succeed in carrying out a plan I have formed for this object. Neither have any observations been made yet which give in any case a determination of the first member; but they may easily be accomplished by any person who possesses a conductor of which the resistance has been determined in absolute measure. Mr. Joule having kindly put me in possession of the silver wire on which his observations of the electrical generation of heat, in 1845, were made with currents measured by a tangent galvanometer used by him about the same time in experimenting on the electrolysis of sulphate of copper and sulphate of zinc, I hope to be able to complete the test of the theoretical result without difficulty, in any case in which I may succeed in determining the amount of the Peltier thermal effect.

117. In the mean time it is interesting to form an estimate,

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however rough, of the absolute values of the thermo-electric elements in any case in which observations that have been made afford, directly or indirectly, the requisite data. This I have done for copper and bismuth, and copper and iron, in the manner shown in the following explanation, which was communicated in full to the Royal Society of Edinburgh when the theory was first brought forward in 1851, although only the part enclosed in double quotation marks was printed in the 'Proceedings.'

118. Example 1. *Copper and Bismuth.*—" 'Failing direct data, the absolute value of the electromotive force in an element of copper and bismuth, with its two junctions kept at the temperatures 0° and 100° Cent., may be estimated indirectly from Pouillet's comparison of the strength of the current it sends through a copper wire 20^m long and 1 millim. in diameter, with the strength of a current decomposing water at an observed rate, by means of the determinations by Weber and others, of the specific resistance of copper and the electro-chemical equivalent of water, in absolute units. The specific resistances of different specimens of copper having been found to differ considerably from one another, it is impossible, without experiments on the individual wire used by M. Pouillet, to determine with much accuracy the absolute resistance of his circuit; but the author has estimated it on the hypothesis that the specific resistance of its substance is $2\frac{1}{4}$ British units. Taking ·02 as the electro-chemical equivalent of water in British absolute units, the author has thus found 16,300 as the electromotive force of an element of copper and bismuth, with the two junctions at 0° and 100° respectively. About 154 of such elements would be required to produce the same electromotive force as a single cell of Daniell's—if, in Daniell's battery, the whole chemical action were electrically efficient*. A battery of 1000 copper and bismuth elements, with the two sets of junctions at 0° and 100° C., employed to work a galvanic engine, if the resistance in the whole circuit be

* M. Jules Regnauld has since found experimentally, that 165 copper-bismuth elements balance the electromotive force of a single cell of Daniell's (see *Comptes Rendus*, Jan. 9, 1854, or *Bibliothèque Universelle de Genève*, March 1854), a result agreeing with the estimate quoted in the text more closely than the uncertainty and indirectness of the data on which that estimate was founded would have justified us in expecting. The comparison of course affords no test of the thermo-electric theory; and only shows, that, as far as the observations of Weber and others alluded to render Pouillet's available for determining the absolute electromotive force of a copper-bismuth element, the absolute electromotive force of a single cell of Daniell's, obtained by multiplying it by the number found by M. Regnauld, agrees with that which I first gave on the hypothesis of all the chemical action being electrically efficient (Phil. Mag. Dec. 1851), and so confirms this hypothesis.

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equivalent to that of a copper wire of about 100 feet long and about one-eighth of an inch in diameter, and if the engine be allowed to move at such a rate as by inductive reaction to diminish the strength of the current to the half of what it is when the engine is at rest, would produce mechanical effect at the rate of about one-fifth of a horse-power. The electromotive force of a copper and bismuth element, with its two junctions at 0° and 1°, being found by Pouillet to be about $\frac{1}{100}$ of the electromotive force when the junctions are at 0° and 100, must be about 163. The value of Θ_0' " [*i. e.* in terms of the notation now used, $\Pi(273{\cdot}7)$, or the value of $\Pi(t)$, for the freezing-point] " 'for copper and bismuth, or the quantity of heat absorbed in a second of time by a current of unit strength in passing from bismuth to copper, when the temperature is kept at 0° C., must therefore be $\frac{163}{160{\cdot}16}$, or very nearly equal to the quantity required to raise the temperature of a grain of water from 0° to 1° C.' "

119. Example 2. *Copper and Iron.*—"By directing the electromotive force of one copper and bismuth element against that of a thermo-electric battery of a variable number of copper and iron wire elements in one circuit, I have found, by a galvanometer included in the same circuit, that when the range of temperature in all the thermo-electric elements is the same, and not very far at either limit from the freezing-point of water, the current passes in the direction of the copper-bismuth agency when only three, and in the contrary direction when four or more of the copper-iron elements are opposed to it. Hence the electromotive force of a copper-bismuth element is between three and four times that of a copper-iron element with the same range of temperature, a little above the freezing-point of water. The electromotive force of a copper-iron element, with its two junctions at 0° and 1° C. respectively, must therefore be something greater than one-fourth of the number found above for copper-bismuth with the same range of temperature, that is, something more than forty British absolute units, and we may consequently represent it by $m \times 40$, where $m > 1$. We have then by the equation expressing the application of Carnot's principle [equation (19) of § 116],

$$\Theta_0 \mu = \Theta_0 \frac{J}{273{\cdot}7} = m \times 40,$$

whence*

$$\Theta_0 = \tfrac{1}{4} m \text{ nearly} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a).$$

* The value of J now used being $32{\cdot}2 \times 1390 = 44{,}758$, which is the equivalent of the unit of heat in "absolute units" of work. The "absolute unit of force" on which this unit of work is founded, and which is generally used in magnetic and electro-magnetic expressions, is the force

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"Now, by the principle of mechanical effect, we have

$$F_0^{280}=J\left(\int_0^{280}\vartheta dt-\Theta_0\right);$$

if F_0^{280} denote the electromotive force of a copper-iron element of which the two junctions are respectively 0° and 280° C., and ϑdt the quantity of heat absorbed per second by a current of unit strength, in passing in copper from a locality at temperature t to a locality at $t+dt$, and in iron from a locality at $t+dt$ to a locality at t*; since the Peltier generation of heat between copper and iron at their neutral point, 280°, vanishes†, and therefore the only absorption of heat is that due to the electric convection expressed by $\int\vartheta dt$; while there is evolution of heat amounting to Θ_0 at the cold junction, and of mechanical effect by the current amounting to F units of work. If we estimate the value of F_0^{280} as half what it would be were the electromotive force the same for all equal differences of temperature as for small differences near the freezing-point‡, that is, if we take $F_0^{280}=\frac{1}{2}\times 40m\times 280$, the preceding equation becomes

$$140\times m\times 40=J\left(\int_0^{280}\vartheta dt-\Theta_0\right).$$

But we found

$$m\times 40=\mu\Theta_0.$$

Hence

$$\int_0^{280}\vartheta dt=\Theta_0\left(1+\frac{140\mu}{J}\right)=\Theta_0\left(1+\frac{140}{272\cdot 7}\right)=\Theta_0\times\frac{3}{2}\text{nearly }(b);$$

or, according to (*a*),

$$\int_0^{280}\vartheta dt=m\times\frac{3}{8}\quad\ldots\ldots\quad(c);$$

results, of which (*b*) shows how the difference of the aggregate amount of the theoretically indicated convective effect in the two metals is related to the Peltier effect at the cold junction; and

which acting on the unit of matter (one grain) during the unit of time (one second), generates a unit of velocity (one foot per second). The "absolute unit of work" is the work done by the absolute unit of force in acting through the unit of space (one foot).

* That is, if ϑ denote the algebraic excess of the specific heat of electricity in copper, above the specific heat of electricity in iron, according to the terms more recently introduced.

† See Proceedings of the Royal Society, Phil. Mag. July 1854. Instead of 240°, conjectured from Regnault's observation when these details were first published, 280° is now taken as a closer approximation to the neutral point of copper and iron.

‡ See Phil. Mag. for July 1854, p. 63.

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(*c*) shows that its absolute value is rather more than one-third of a thermal unit per second per unit strength of current.

120. If the specific heats of current electricity either vanished or were equal in the different metals, we should have by (15) and (16),

$$\frac{\Pi}{t} = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20),$$

and

$$F = J\frac{\Pi}{t}(T - T') \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21),$$

or the Peltier thermal effect at a junction of two metals would be proportional to the absolute temperature at which it takes place, and the electromotive force in a circuit of any two metals would vary in the simple ratio of the difference of temperature on the new absolute scale between their junctions*. Whatever thermometric system be followed, the second of these conclusions would require the same law of variation of electromotive force with the temperatures of the junctions in every pair of metals used as a thermo-electric element.

121. Before the existence of a convective effect of electricity in an unequally heated metal had even been conjectured, I arrived at the preceding conclusions by a theory in which the Peltier effect was taken as the only thermal effect reversible with the current in a thermo-electric circuit, and found them at variance with known facts which show remarkably different laws of electromotive force in thermo-electric pairs of different metals. I therefore inferred, that, besides the Peltier effect, there must be other reversible thermal effects; and I showed that these can be due to no other cause than the inequalities of temperature in single metals in the circuit. A convective effect of electricity in an unequally heated conductor of one metal was thus first demonstrated by theoretical reasoning; but only the difference of the amount of this effect produced by currents of equal strength in different metals, not its quality or its absolute value in any one metal, could be inferred from the data of thermo-electric force alone. The case of a thermo-electric circuit of copper and iron, being that which first forced on me the conclusion that an

* When the theory was first communicated to the Royal Society of Edinburgh, I stated these conclusions with reference to temperature by the air thermometer, and therefore in terms of Carnot's absolute function of the temperature, not simply, as now, in terms of absolute temperature. At the same time I gave, as consequences of Mayer's hypothesis, the same statement in terms of air-thermometer temperatures, as is now made absolutely. See 'Proceedings,' Dec. 15, 1851; or Philosophical Magazine, vol. iii. p. 532.

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electric current must produce different effects according as it passes from hot to cold, or from cold to hot, in an unequally heated metal, was taken as an example in my first communication of the theory to this Society*; and the two metals, copper and iron, were made the subjects of a consequent experimental investigation, to ascertain the quality of the anticipated property in each of them separately. The application of the general reasoning to this particular case, and the answers which I have derived by experiment to the question which it raises, are described (§§ 122–133) in a Report communicated to the Royal Society of London, March 31, and contained in the 'Proceedings' published in this Magazine for July 1854.

§§ 134, 135. *Inserted September* 15, 1854.

134. A continuation of the experiments has shown many remarkable variations of order in the thermo-electric series. The following Table exhibits the results of observations to determine neutral points for different pairs of metals; the number at the head of each column being the temperature Centigrade at which the two metals written below it are thermo-electrically neutral to one another; and the lower metal in each column being that which *passes the other from bismuth towards antimony* as the temperature rises.

−14° C.	−12°·2.	−1°·5.	8°·2.	36°.	38°.	44°.	44°.	64°.	99°.	121°.	130°.	162°·5.	237°.	280°.
P_3	P_1	P_1	P_1	P_2	P_2	P_2	Lead	P_1	P_1	P_1	P_1	Iron	Iron	Iron
Brass	Cadmium	Silver	Zinc	Lead	Brass	Tin	Brass	Copper	Brass	Lead	Tin	Cadmium	Silver	Copper

I also found that brass becomes neutral to copper, and copper becomes neutral to silver, at some high temperatures, estimated at from 800° to 1400° Cent. in the former case, and from 700° to 1000° in the latter, being a little below the melting-point of silver. The following diagram exhibits the results graphically, constructed on the principle of drawing a line through the letters corresponding to any one of the metallic specimens in a table such as that of § 130†, and arranging the spaces so that each line shall be as nearly straight as possible, if not exactly so.

* See Proceedings of the Royal Society of Edinburgh, Dec. 15, 1851; or Phil. Mag. vol. iii. p. 529, 1852.

† Phil. Mag. for July 1854, p. 67.

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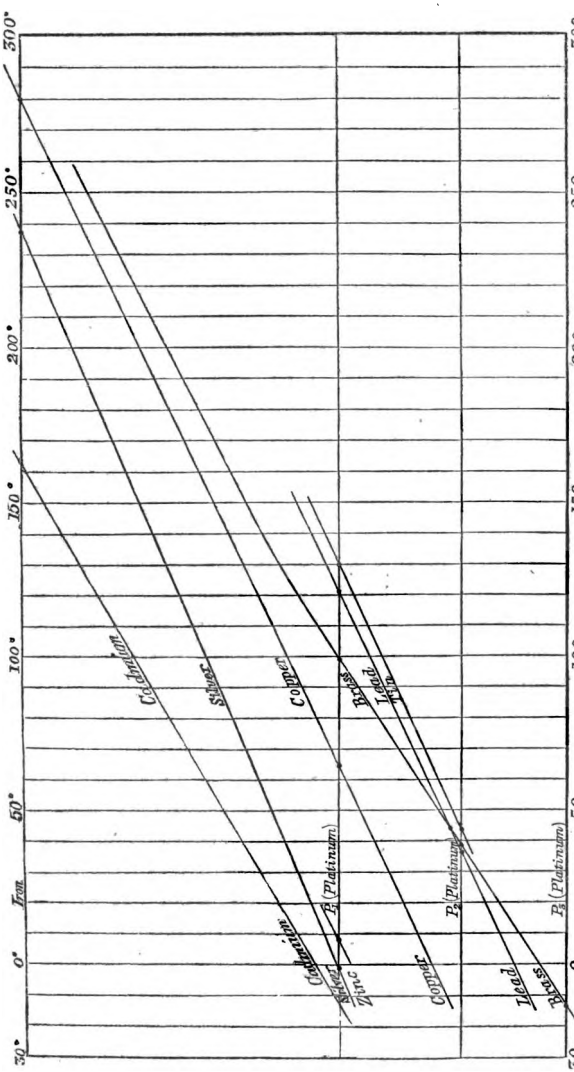

Explanation of Thermo-electric Diagram.

The orders of the metals in the thermo-electric series at different temperatures are shown by the points at which the vertical lines through the numbers expressed by the temperatures Centigrade are cut by the horizontal and oblique lines named for the different metallic specimens.

P_1, P_2, P_3 denote three platinum wires found to differ thermo-electrically, and used as standards.

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The object to be aimed at in perfecting a thermo-electric diagram, and perhaps approximately attained to (conjecturally) in the preceding, is to make the ordinates of the lines (which will in general be curves) corresponding to the different metallic specimens, be exactly proportional to their *thermo-electric powers**, with reference to a standard metal (P_3 in the actual diagram).

135. Judging by the eye from the diagram, as regards the convective agency of electricity in unequally heated conductors, I infer that the different metals are probably to be ranked as follows, in order of the values of the specific heat of electricity in them:—

Specific Heat of Vitreous Electricity.

In	Cadmium	Positive.
...	Brass	...
...	Copper	...
...	Lead, Tin, Silver . . .	Positive, zero, or negative.
...	Platinum	Probably negative.
...	Iron	Negative.

Zinc probably stands high, certainly above platinum.

136. A very close analogy subsists between the thermodynamical circumstances of an electrical current in a circuit of two metals, and those of a fluid circulating in a closed rectangular tube, consisting of two vertical branches connected by two horizontal branches. Thus if, by the application of electromotive force in one case, or by the action of pistons in the other, a current be instituted, and if at the same time the temperature be kept uniform throughout the circuit, heat will be evolved and absorbed at the two junctions respectively in the former case, and heat will be evolved in one and absorbed in the other of the vertical branches of the tube in the latter case, in consequence of the variations of pressure experienced by the fluid in moving through those parts of the circuit. If the temperature of one junction of the electrical circuit be raised above that of the other, and if the temperature of one vertical branch of the tube containing fluid be raised above that of the other, a current will in each case be occasioned without any other motive appliance. If the current be directed to do work with all its energy, by means of an engine in each case, there will be a conversion of heat into mechanical effect, with perfectly analogous relations as to absorption and evolution of heat in different parts of the circuit, provided the engine worked by the fluid current be arranged to pass the fluid through it without variation of temperature from or to either of the vertical branches of the tube. If σ_1 and σ_2

* See § 140, below.

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denote the specific heat of unity of mass of the fluid under the constant pressures at which it exists in the lower and upper horizontal branches of the tube in the second case; $\Pi(T)$, $\Pi(T')$ the quantities of heat evolved and absorbed respectively by the passage of a unit mass of fluid through the two vertical branches kept at the respective temperatures T, T'; and if F denote the work done by a unit mass of the fluid in passing through the engine; the fundamental equations obtained above with reference to the thermo-electric circumstances, may be at once written down for the case of the ordinary fluid as the expression of the two fundamental laws of the dynamical theory of heat, both of which are applicable to this case, without any uncertainty such as that shown to be conceivable as regards the application of the second law to the case of a thermo-electric current. The two equations thus obtained are equivalent to the two general equations given in §§ 20 and 21 of the first part of this series of papers, as the expressions of the fundamental laws of the dynamical theory of heat applied to the elasticity and expansive properties of fluids. In fact, when we suppose the ranges of both temperature and pressure in the circulating fluid to be infinitely small, the equation $F=J\int_{T'}^{T}\frac{\Pi}{t}\,dt$, reduced to the notation formerly used, and modified by changing the independent variables from (t,p) to (t,v), becomes

$$M=\frac{t}{J}\frac{dp}{dt},$$

which is the same as (3) of § 21; and a combination of this with $\frac{d}{dt}\left(\frac{\Pi}{t}\right)=\frac{\sigma_2-\sigma_1}{t}$, gives

$$\frac{dM}{dt}-\frac{dN}{dv}=\frac{1}{J}\frac{dp}{dt},$$

which is identical with (2) of § 20. It appears, then, that the consideration of the case of fluid motion here brought forward as analogous to thermo-electric currents in non-crystalline linear conductors, is sufficient for establishing the general thermodynamical equations of fluids; and consequently the universal relations among specific heats, elasticities, and thermal effects of condensation or rarefaction, derived from them in Part III., are all included in the investigation at present indicated. Not going into the details of this investigation, because the former investigation, which is on the whole more convenient, is fully given in Parts I. and III., I shall merely point out a special application of it to the case of a liquid which has a temperature of maximum density, as for instance water.

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137. In the first place, it is to be remarked that if the two vertical branches be kept at temperatures a little above and below the point of maximum density, no current will be produced; and therefore if T_0 denote this temperature, the equation $F=\int\frac{\Pi}{t}\,dt$ gives $\Pi(T_0)=0$. Again, if one of the vertical branches be kept at T_0, and the other be kept at a temperature either higher or lower, a current will set, and always in the same direction. Hence $\int_{T_0}^{T}\frac{\Pi}{t}\,dt$ has the same sign, whether T be greater or less than T_0, and consequently $\Pi(t)$ must have contrary signs for values of t above and below T_0; which, by attending to the signs in the general formulæ, we see must be such as to express evolution of heat by the actual current in the second vertical branch when its temperature is below, and absorption when above T_0. As the current in each case ascends in this vertical branch, we conclude that a slight diminution of pressure causes evolution or absorption of heat in water, according as its temperature is below or above that of maximum density; or conversely, that when water is suddenly compressed, it becomes colder if initially below, or warmer if initially above, its temperature of maximum density. This conclusion from general thermo-dynamic principles was first, so far as I know, mentioned along with the description of an experiment to prove the lowering of the freezing-point of water by pressure, communicated to the Royal Society in January 1850*. The quantitative expression for the effect, which was given in § 50 of Part III., may be derived with ease from the considerations now brought forward. The other thermo-dynamic equation $\frac{\sigma_2-\sigma_1}{t}=\frac{d\left(\frac{\Pi}{t}\right)}{dt}$ shows that the specific heat of the water must be greater in the upper horizontal branch than in the lower, or that the specific heat of water under constant pressure is increased by a diminution of the pressure. The same conclusion, and the amount of the effect, are also implied in equations (18) and (19) of Part III. We may arrive at it without referring to any of the mathematical formulæ, merely by an application of the general principle of mechanical effect, when once the conclusion regarding the thermal effects of condensation or rarefaction is established; exactly as the conclusion regarding the specific heats of electricity in copper and in iron was first arrived at†. For if we suppose one vertical branch to be kept

* See 'Proceedings' of that date, or Philosophical Magazine, 1850.

† Proceedings of the Royal Society of Edinburgh, Dec. 15, 1851; or extract of Proceedings of the Royal Society, May 1854.

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at the temperature of maximum density (corresponding to the neutral point of the metals in the corresponding thermo-electric case), and the other at some lower temperature, a current will set downwards through the former branch, and upwards through the latter. This current will cause evolution of heat, in consequence of the expansion of the fluid, in the branch through which it rises, but will cause neither absorption nor evolution in the other vertical branch, since in it the temperature is that of the maximum density. There will also be heat generated in various parts by fluid friction. There must then be, on the whole, absorption of heat in the horizontal branches, because otherwise there would be no source of energy for the heat constantly evolved to be drawn from. But heat will be evolved by the fluid in passing in the lower horizontal branch from hot to cold; and therefore, exactly to the extent of the heat otherwise evolved, this must be over-compensated by the heat absorbed in the upper horizontal branch by the fluid passing from cold to hot. On the other hand, if one of the vertical branches be kept above the temperature of maximum density and the other at this point, the fluid will sink in the latter, causing neither absorption nor evolution of heat, and rise in the former, causing absorption; and therefore more heat must be evolved by the fluid passing from hot to cold in the upper horizontal branch than is absorbed by it in passing from cold to hot in the lower. From either case we infer that the specific heat of the water is greater in the upper than in the lower branch. The analogy with the thermo-electric circumstances of two metals which have a neutral point is perfect algebraically in all particulars. The proposition just enunciated corresponds exactly to the conclusion arrived at formerly, that if one metal passes another in the direction from bismuth towards antimony in the thermo-electric scale, the specific heat of electricity is greater in the former metal than in the latter; this statement holding algebraically, even in such a case as that of copper and iron, where the specific heats are of contrary sign in the two metals, although the existence of such contrary effects is enough to show how difficult it is to conceive the physical circumstances of an electric current as physically analogous to those of a current of fluid in one direction.

§§ 138–140. *General Lemma, regarding relative thermo-electric properties of Metals, and multiple combinations in a Linear Circuit.*

138. The general equation (11), investigated above, shows that *the aggregate amount of all the thermal effects produced by a current, or by any system of currents, in any solid conductor or*

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combination of solid conductors must be zero, if all the localities in which they are produced are kept at the same temperature.

Cor. 1. If in any circuit of solid conductors the temperature be uniform from a point P through all the conducting matter to a point Q, both the aggregate thermal actions and the electromotive force are totally independent of this intermediate matter, whether it be homogeneous or heterogeneous, crystalline or non-crystalline, linear or solid, and is the same as if P and Q were put in contact. [The importance of this simple and elementary truth in thermo-electric experiments of various kinds is very obvious. It appears to have been overlooked by many experimenters, who have scrupulously avoided introducing extraneous matter (as solder) in making thermo-electric junctions, and who have attempted to explain away Cumming's and Becquerel's remarkable discovery of thermo-electric inversions, by referring the phænomena observed to coatings of oxide formed on the metals at their surfaces of contact.]

Cor. 2. If $\Pi(A, B)$, $\Pi(B, C)$, $\Pi(C, D)$, ... $\Pi(Z, A)$ denote the amounts of the Peltier absorption of heat per unit of strength of current per unit of time, at the successive junctions of a circuit of metals A, B, C, ... Z, A, we must have

$$\Pi(A, B)+\Pi(B, C)+ \ldots . +\Pi(Z, A)=0.$$

Thus if the circuit consist of three metals,

$$\Pi(A, B)+\Pi(B, C)+\Pi(C, A)=0;$$

from which, since $\Pi(C, A)=-\Pi(A, C)$, we derive

$$\Pi(B, C)=\Pi(A, C)-\Pi(A, B).$$

139. Now, by (19) above, the electromotive force in an element of the two metals (A, B), tending from B to A through the hot junction, for an infinitely small difference of temperature τ, and a mean absolute temperature t, is $\frac{J\Pi(A, B)}{t}\tau$, and so for every other pair of metals. Hence, if $\phi(A, B)$, $\phi(B, C)$, &c. denote the quantities by which the infinitely small range τ must be multiplied to get the electromotive forces of elements composed of successive pairs of the metals in the same thermal circumstances, we have

$$\phi(A, B)+\phi(B, C)+ \ldots . +\phi(Z, A)=0;$$

and for the case of three metals,

$$\phi(B, C)=\phi(A, C)-\phi(A, B).$$

Since the thermo-electric force for any range of temperature is the sum of the thermo-electric forces for all the infinitely small ranges into which we may divide the whole range (being, as

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proved above, equal to $\int_T^{T'} \phi dt$), in the case of each element, the theorem expressed by these equations is true of the thermo-electric forces in the single elements for *all ranges* of temperature, provided the absolute temperatures of the hot and cold junctions be the same in the different elements. The second equation, by successive applications of which the first may be derived, is the simplest expression of a theorem which was, I believe, first pointed out and experimentally verified by Becquerel in researches described in the second volume of his *Traité d'Electricité.*

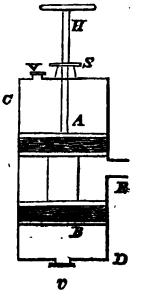
140. For brevity, we shall call what has been denoted by ϕ(B, C) *the thermo-electric relation* of the metal B to the metal C; we shall call a certain metal (perhaps copper or silver) the standard metal; and if A be the the standard metal, we shall call ϕ(A, B) the *thermo-electric power of the metal B.* The theorem expressed by the last equation may now be stated thus:—*The thermo-electric relation between two metals is equal to the difference of their thermo-electric powers,* which is nearly identical with Becquerel's own statement of his theorem.

[To be continued.]

XXXVII. *On a New Double-acting Air-pump with a single Cylinder.* By T. Tate, *F.R.A.S.**

THE chief points of novelty in this air-pump consist in a double piston acting in a single cylinder, and in the superior system of valves. This double piston with a single cylinder gives to the instrument all the properties of an ordinary air-pump with two cylinders: this new instrument, in fact, may be regarded as a single-barrelled pump, capable of performing its work with only one-half the usual motion.

The annexed diagram represents a section of the cylinder, &c. of my new air-pump: CD the cylinder; A and B solid pistons rigidly connected by a rod, and moved by the piston-rod AH, passing through a stuffing-box S; V and *v* valves lifting outwards; R an open pipe at the middle of the cylinder leading to the receiver from which the air is to be exhausted. The distance between the extreme faces of the pistons is about $\frac{3}{8}$ths of an inch less than one-half the length of the cylinder, this $\frac{3}{8}$ths of an inch being the space requisite for clearing the exhausting pipe R. The pistons are each about $1\frac{1}{2}$ inch in thickness, and



* Communicated by the Author.

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the rod connecting them may be of any section consistent with strength. The effective length of the stroke is equal to the space between one side of the pipe R and the corresponding end face of the cylinder, or it is very nearly equal to one-half the length of the cylinder.

In an upward stroke, the air above the piston A is propelled through the valve V into the atmosphere, while a vacuum is being formed beneath the piston B. When the piston A strikes against the top of the cylinder, the air from the receiver rushes through the pipe R and diffuses itself through the lower half of the cylinder. In a downward stroke, the air beneath the piston B is propelled through the valve v into the atmosphere, while a vacuum is being formed above the piston A; and so on. It will be observed that *the double piston performs a double duty at every single stroke*; for while a vacuum is being formed on one side of the cylinder by one piston, the other piston is propelling air from the opposite side of the cylinder into the atmosphere.

With the view of calculating the exhausting power of this air-pump, let a = the volume of the air in the receiver; b = the volume of the air discharged at each stroke; b_1 = the volume of the air between the piston and the valve at the end of each stroke; E = the elasticity of the external air, measured by a column of mercury; eE = the elasticity of the air required to lift the valves; and n = the number of strokes: then we have

$$\mathrm{E}_1(a+b+b_1)=a\mathrm{E}+b_1e\mathrm{E}, \text{ and } \therefore\ \mathrm{E}_1=\frac{a\mathrm{E}+b_1e\mathrm{E}}{a+b+b_1};$$

and generally $\mathrm{E}_n=\dfrac{a\mathrm{E}_{n-1}+b_1e\mathrm{E}}{a+b+b_1}$; hence we find

$$\mathrm{E}_n=\left(\frac{a}{a+b+b_1}\right)^n\mathrm{E}+\frac{b_1e\mathrm{E}}{a+b+b_1}\left\{1+\frac{a}{a+b+b_1}+\ldots.+\left(\frac{a}{a+b+b_1}\right)^{n-1}\right\}$$

$$=\left(\frac{a}{a+b+b_1}\right)^n\mathrm{E}\left(1-\frac{b_1e}{b+b_1}\right)+\frac{b_1e\mathrm{E}}{b+b_1}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This expression gives the elasticity of the air in the receiver at the end of the nth stroke.

When the number of strokes is indefinitely increased, or when $n=\infty$, or what is equivalent in result, when a is very small as compared with b, we have

$$\mathrm{E}_\infty=\frac{b_1e\mathrm{E}}{b+b_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which expresses the limit of exhaustion.

In the common pump, the limit of exhaustion for the cylinder is the same as for the receiver of the new pump; let E′ be put for the elasticity of the air in the cylinder at this limit, and E″ for the elasticity of the air in the receiver at the limit of ex-

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haustion; then

$$E''=E'+E(e-1)=\frac{b_1 eE}{b+b_1}+E(e-1), \quad . \quad . \quad (3)$$

which expresses the limit of exhaustion for the receiver of the common air-pump. The elasticity of the air in this case is greater than the limiting elasticity of the air in the receiver of the new pump by the fraction $E(e-1)$.

Assuming $a=108$ cub. inch.; $b=12$ cub. inch.; $b_1=\frac{1}{60{\cdot}5}$ cub. inch.; $E=30$ inch.; $e=\frac{121}{120}$ (which gives about a pressure of 10 grs. on a valve orifice of $\frac{1}{10}$th of an inch in diameter); and $n=60$; then by formula (1) $E_{60}=\cdot095$; that is to say, the elasticity of the air in the receiver of the new pump at the end of sixty single strokes will be measured by about $\frac{1}{10}$th of an inch of mercury. Again, by formula (2) we find the limit of exhaustion to be measured by $\cdot041$ inch of mercury, or about $\frac{1}{24}$th of an inch of mercury.

Now by formula (3) we have

$$E''=\cdot041+\cdot25=\cdot291, \text{ or } \cdot3 \text{ nearly};$$

that is to say, the limit of exhaustion for a common pump in this case will be measured by $\frac{3}{10}$ths of an inch of mercury, being $\frac{1}{4}$th of an inch in excess of that derived for the new pump.

This pump, therefore, not only exhausts the air more rapidly than the common pump, but it also carries that exhaustion to a much higher limit.

These theoretical calculations have been fully confirmed by actual experiment. I have had a pump made on this new principle, with a stroke of 8 inches, and $1\frac{1}{2}$ inch in the section*, and I have compared it with a superior pump on the old construction: the following is an exact statement of the results of my experiments.

Results of experiments with the new Pump.

Capacity of the receiver 108 cubic inches. Common siphon gauge.

Elasticity of the air in the receiver at the end of sixty *single strokes* $=\cdot12$ inch of mercury. Limit of exhaustion $=\cdot05$ inch of mercury.

Results of experiments with a common double-barrelled Pump, EACH PISTON *having a stroke of 4 inches.*

Capacity of the receiver, &c. as before.

Elasticity of the air in the receiver at the end of 120 strokes of the two pistons $=\cdot6$ inch of mercury.

Limit of exhaustion $=\cdot3$ inch of mercury.

* This instrument may be seen at Messrs. Murray and Heath's, Philosophical Instrument Makers, Piccadilly.

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The advantages of this new pump, as compared with the common pump of the same capacity of cylinder, are as follows:—

1. To effect the same exhaustion, the driving pressure moves over one-half the space.

2. From the superior construction of valves, the exhaustion is carried to a much greater extent.

3. From having a double stroke in a single cylinder, the expense of construction is considerably reduced.

4. As the exhaustion proceeds, the pressure requisite for moving the pistons becomes less and less. The contrary takes place in the common pump.

In the pump which I have constructed the valves are made of oil-silk, and the piston-rod is moved by the direct application of the pressure; but I purpose to construct a pump on the new principle, with metal valves covered with oil-cisterns, and lifted up by the stroke of the piston, and also with a pump-lever (or crank) attached to the head of the piston-rod. The pump thus constructed will most certainly exhaust the air from the receiver until its elasticity is reduced to that of the vapour of the oil.

Hounslow, March 20, 1856.

XXXVIII. *Notices respecting New Books.*

The Geometry of the Three First Books of Euclid, by Direct Proof, from Definitions alone, with an Introduction on the Principles of the Science. By Henslegh Wedgwood, *M.A., late Fellow of Christ College, Cambridge.* 12mo. London: Walton and Maberly. 1856.

MODERN mathematicians have been so much occupied in investigating the extent and value of the higher analysis, that elementary geometry has been very much neglected. Dr. Whewell, Professor De Morgan, Mr. Sylvester, Mr. Wedgwood, and a few others, have occasionally done something on the subject of geometrical reasoning, but, as a general rule, with more of a speculative than of a practical spirit. The work now before us has the advantage of exemplifying the author's views with actual demonstrations. In this respect it is far superior to anything Mr. Wedgwood has done before. The main points in his system have been given from time to time in our notices of the Cambridge Philosophical Society. This alone prevents us from entering into any detailed account of a work so well deserving of attention.

Mr. Wedgwood traces out with much care the ultimate expression of the mode in which the fundamental conceptions of geometry are brought into intellectual existence. He supports the views of Locke and Dugald Stewart, by making the demonstrations depend solely on definitions, and he shows the practical possibility of altogether excluding *reductio ad absurdum* from geometrical reasoning. His introduction is well worth reading. It is marked with much originality, and is written in a clear and logical style.

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XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 237.]

June 21, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"On Axes of Elasticity and Crystalline Forms." By W. J. Macquorn Rankine, C.E., F.R.SS.L. & E. &c.

An Axis of Elasticity is any direction with respect to which any kind of elastic force is symmetrical.

In this paper the deviation of a molecule of a solid from that condition as to volume and figure which it preserves when free from the action of external forces, is denoted by the word "*Strain*," and the corresponding effort of the molecule to recover its free volume and figure by the word "*Stress*."

In devising a nomenclature for quantities relating to the theory of elasticity, *strain* is denoted in composition by θλίψις, and *stress* by τάσις.

Every possible strain of a molecule, when referred to rectangular axes, may be resolved into six *elementary strains*; three elongations or linear compressions, and three distortions. Every possible stress, when referred to rectangular axes, may be resolved into six *elementary stresses*; three normal pressures, and three tangential pressures, which tend to diminish the corresponding elementary strains.

The elementary strains being in fact approximately linear functions of the elementary stresses, are treated in this investigation as exactly so.

The sum of the six integrals of the elementary stresses, each taken with respect to the corresponding elementary strain *from* its actual amount *to* zero, is the *Potential Energy of Elasticity*, and is a homogeneous function of the elementary strains of the second order, and of twenty-one terms, whose constant coefficients are here called the *Tasinomic Coefficients*, or coefficients of Elasticity.

The principles of the Calculus of Forms, and in particular the *Umbral Notation* of Mr. Sylvester, are applied to the Orthogonal Transformations of the Tasinomic Coefficients.

Several functions of these coefficients are determined, called *Tasinomic Invariants*, which are equal for all systems of orthogonal axes in the same solid.

Certain functions of the Tasinomic Coefficients constitute the coefficients of two *Tasinomic Ellipsoids*, designated respectively as the *Orthotatic* and *Heterotatic* Ellipsoids, whose axes have the following properties.

Orthotatic Axes.

At each point of an elastic solid there is one position in which a cubical molecule may be cut out, such, that a uniform dilatation or condensation of that molecule by equal elongations or compressions of its three axes, will produce no tangential stress at the faces of the molecule.

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The existence of orthotatic axes in a solid constituted of mutually attracting and repelling physical points was first proved by Mr. Haughton; it is proved in this paper independently of any hypothesis as to molecular structure or action.

Heterotatic Axes.

At each point of an elastic solid there is one position in which a cubical molecule may be cut out, such, that if there be a distortion of that molecule round x (x *being any one of its axes*) *and an equal distortion round* y (y *being either of its other two axes*), *the normal stress on the faces normal to* x *arising from the distortion round* x, *will be equal to the tangential stress around* z *arising from the distortion round* y.

The six coefficients of the Heterotatic Ellipsoid represent parts of the elasticity of a solid which it is impossible to reduce to attractions and repulsions between points.

Fifteen constants called the *Homotatic Coefficients*, which are composed of Tasinomic Coefficients and their linear functions so constituted as to be independent of the *Heterotatic Coefficients*, are the coefficients of the fifteen terms of a homogeneous biquadratic function of the co-ordinates, which being equated to unity, characterizes the *Biquadratic Tasinomic Surface*. This surface, for solids composed of physical points, was discovered by Mr. Haughton; it is here investigated independently of all hypothesis.

By rectangular linear transformations, three functions of the Homotatic Coefficients may be made to vanish. Three orthogonal axes are thus found, which are called the *Principal Metatatic Axes*, and have the following property: *if there be a linear elongation along any one of these axes, and an equal linear compression along any other, no tangential stress will result on planes normal to these two axes.*

In each of the three planes of the principal Metatatic Axes, there is a pair of Diagonal Metatatic Axes bisecting the right angles formed by the pair of principal axes in the same plane.

In each plane in an elastic solid, there is a system of two pairs of metatatic axes, making with each other eight equal angles of 45°.

Various kinds and degrees of symmetry are pointed out, which the tasinomic coefficients may have with respect to orthogonal axes.

The Potential Energy of Elasticity may be expressed as a homogeneous function of the second order of the Elementary Stresses. The twenty-one coefficients of this function are called Thlipsinomic Coefficients.

The Thlipsinomic and Tasinomic Coefficients are related to each other as Contragredient Systems.

The Orthogonal and Diagonal Tasinomic and Thlipsinomic Axes coincide.

For the complete determination of the properties of the Homotatic Coefficients, it is necessary to refer them to oblique axes of co-ordinates.

The application of oblique co-ordinates to this purpose is much facilitated by the employment along with them of three auxiliary

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variables called *Contra-ordinates*. The contra-ordinates of a point R are the projections of the radius-vector OR on the three axes. For rectangular axes, co-ordinates and contra-ordinates are identical. The co-ordinates x, y, z and contra-ordinates u, v, w of a point R are connected by the equation

$$ux+vy+wz=\overline{OR}^2.$$

As there are six independent quantities in the directions of a system of three axes of indefinite obliquity, there is a system of right or oblique axes in every solid for which six of the coefficients of the characteristic function of the Biquadratic surface disappear, reducing that function to its canonical form of nine terms. These three axes are called the

PRINCIPAL EŬTHYTATIC AXES.

Every Eŭthytatic axis has this property, *that a direct linear elongation or compression along such an axis, produces a normal stress, and no oblique or tangential stress on a plane normal to the same axis.*

Every Eŭthytatic axis is normal to the Biquadratic Surface, and is a line along which the direct elasticity of the body is either a maximum or a minimum, or in that condition which combines the properties of maximum and minimum.

It is probable that the faces or edges of primitive crystalline forms are normal to Eŭthytatic axes, and that the planes of cleavage in crystals are normal to Eŭthytatic axes of minimum elasticity.

It is also probable that the symmetrical summits of crystals correspond to Eŭthytatic axes.

There are, in every solid, at least the three principal Eŭthytatic Axes, which are normal to the faces of a hexahedron, right or oblique as the case may be. In certain cases of symmetry of these axes and of the Homotatic Coefficients, there are *Secondary or Additional Eŭthytatic Axes*, which are determined by the following principles:

1. When the three principal axes and the Homotatic Coefficients are symmetrical round a central axis, that axis is an additional Eŭthytatic axis.

2. When there are a pair of orthogonal Eŭthytatic axes in a given plane, there may be, under certain conditions specified in the paper, a pair of *additional or secondary axes* in that plane, making with each other a pair of angles bisected by the orthogonal axes.

In the first column of a table, the possible systems of Eŭthytatic axes are arranged according to a classification and nomenclature of their degrees and kinds of symmetry; and in the second and third columns are stated the primitive crystalline forms to the faces and edges of which such systems of axes are respectively normal, and which embrace all the primitive forms known in crystallography.

The six Heterotatic Coefficients are independent of the fifteen Homotatic Coefficients which determine the Eŭthytatic axes.

The paper concludes with observations on some real and alleged differences between the laws of solid elasticity and those of the luminiferous force,—on some hypotheses in connexion with the wave-theory of light,—and on the refraction of light in crystals as connected with the symmetry of their Eŭthytatic axes.

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December 6.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

The following communication was read:—

"On the Determination of the Dew-point by means of the Dry- and Wet-bulb Thermometers." In a Letter of Lieut. Noble, R.N., of Toronto, to Charles R. Weld, Esq.

Toronto, September 10th, 1855.

MY DEAR SIR,—The results of the accompanying table for computing the dew-point from readings of the dry- and wet-bulb thermometers, are, as I believe you know, derived from observations taken here during last winter by Mr. Campbell and myself:—

TABLE *for computing the Dew-point from Readings of the Dry- and Wet-Bulb Thermometers.*

Temperature of air (t).	Factor (f).	Number of observations (m).	Probable error of a single datum (r).	Measure of precision of a single datum (h).	Probable error of the adopted factor $R=\frac{r}{\sqrt{m}}$.	Measure of precision of the adopted factor $H=h\sqrt{m}$.	It is therefore an equal chance that the true factor lies between
48° to 51°	2·31	21	·30	1·590	·07	7·287	2·24 and 2·38
46 ... 47	2·38	13	·26	1·822	·07	6·569	2·31 ... 2·45
42 ... 45	2·53	41	·40	1·189	·06	7·613	2·47 ... 2·59
40 ... 41	2·63	17	·41	1·163	·10	4·796	2·53 ... 2·73
38 ... 39	2·83	25	·48	0·999	·09	4·994	2·74 ... 2·92
34 ... 37	3·02	64	·43	1·114	·05	8·912	2·97 ... 3·07
32 ... 33	3·33	25	·63	·767	·12	3·835	3·21 ... 3·45
30 ... 31	3·81	22	·61	·775	·16	3·633	3·65 ... 3·97
28 ... 29	4·40	27	·66	·723	·13	3·756	4·27 ... 4·53
24 ... 27	5·46	43	·82	·577	·13	3·787	5·33 ... 5·59
22 ... 23	6·06	15	1·20	·397	·31	1·535	5·75 ... 6·37
20 ... 21	6·93	6	1·40	·341	·57	·834	6·36 ... 7·50
18 ... 19	7·13	21	1·44	·331	·31	1·517	6·82 ... 7·44
16 ... 17	7·60	20	1·76	·271	·39	1·209	7·21 ... 7·99
14 ... 15	8·97	17	1·72	·277	·42	1·141	8·55 ... 9·39
12 ... 13	10·30	20	2·53	·188	·56	·842	9·74 ... 10·86
10 ... 11	11·50	11	2·19	·218	·66	·723	10·84 ... 12·16
8 ... 9	13·06	8	4·63	·103	1·64	·292	11·42 ... 14·70
6 ... 7	15·30	7	3·66	·130	1·38	·345	13·92 ... 16·68
0 ... 5	16·23	14	1·87	·255	·50	·955	15·73 ... 16·73
−1 ... −4	19·37	10	4·11	·116	1·30	·367	18·07 ... 20·67
−5 ... −10	21·64	6	4·65	·102	1·90	·251	19·74 ... 23·54
−11 ... −16	37·83	6	10·96	·044	4·48	·107	33·35 ... 42·31

These results will be obvious at a glance; but a few remarks upon the instruments employed, and upon the degree of reliance to be placed upon them, may not be uninteresting.

The dry- and wet-bulb thermometers (for which we were indebted to the kindness of Prof. Cherriman, Director of the Magnetic Observatory, Toronto) were made by Negretti and Zambra, and their index errors were ascertained, above 32° by Mr. Glaisher, and below 32° by ourselves, by comparison with a Kew standard. The divisions upon these thermometers were too small to read 0°·1 with great accuracy; and in discussing our observations at low temperatures, we were in consequence obliged to reject such as would, with

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an error of 0°·1 in the reading, introduce a considerable error into the factor.

You will observe that the table does not extend below −16°, although we have repeatedly every winter the mercury below −20°, and occasionally below −30°. The only thermometer, however, which we could trust as a wet-bulb in investigations so delicate was not graduated below −16°.

For obtaining the dew-point by direct observation, we used the condensing hygrometer invented by M. Regnault.

We obtained dew with this beautiful instrument at all temperatures (limited only by the graduation of the thermometer −35°), the only requisites when the thermometer is very low being time and pure ether*. I can testify from experience that this hygrometer obviates all the disadvantages of Daniell's, which M. Regnault enumerates in his hygrometrical researches.

In order to show the reliance that may be placed upon our results, we have put opposite each factor in the table the probable error and measure of precision of the single data (from which the factor (f) was derived), and also the probable error, measure of precision, and limits of certainty of the adopted factor. The nomenclature and notation are thus employed by Encke in his Memoir on the Method of Least Squares.

The measure of precision (h), as was indeed to have been expected, decreases with the temperature. This fact is not however of so much importance as might at first appear.

For the dew-point is given by the equation,—

$$T=t-f(t-t'),$$

where (T) is the temperature of the dew-point, (t) that of the air, ($t-t'$) the difference between the dry- and wet-bulb thermometers, and (f) the factor whose value is given in the table.

Now taking the temperatures 42° and 22°, it appears from the table that the probable error of (f) for a single observation is at the latter temperature three times greater than at the former. But ($t-t'$) is on an average about three times as great at 42° as at 22°. Hence the probable error of the dew-point at both temperatures is very nearly the same.

We have extended our table to 51° for the purpose of comparison with the "Greenwich factors." I must however remark, that it is probable that the factors, which we have given above 40°, are rather greater than they would have been had the observations discussed extended through a longer space of time, the majority at these temperatures having been taken last spring, when the air was very remarkably dry; and experience shows that when ($t-t'$) is unusually great, the deduced factor, instead of being more accurate, is generally much too large.

As an instance, I may cite an observation taken on April 29th, when the temperature of the air was 43°·6, that of evaporation was

* The ether we employed below −20° was the first that passed over, resulting from the distillation of washed ether with quicklime.

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31°·6, and that of the dew-point 3°·2. The fraction of saturation on this occasion was $\frac{19}{100}$, and the factor derived from this observation was 3·36; this being much the largest deviation from the adopted mean 2·53.

The cause of this discrepancy is doubtless owing to the heat that the wet-bulb thermometer derives from the radiation of surrounding objects; and were observations sufficiently numerous, it might conduce to accuracy were the factors calculated for every degree of difference in the value of $(t-t')$.

We purpose instituting a comparison between two wet-bulb thermometers placed in similar boxes, the one box coated with lamp-black, the other with a polished metallic surface.

Below 32° our results do not appear to coincide with the factors deduced from the Greenwich observations; and the causes of these discrepancies I must leave to time.

As, however, we have had considerable experience at these temperatures, I may perhaps be doing service to observers in bringing before their notice two causes of error, to which we have found ourselves particularly liable when the thermometer is near 32°.

1st. If the air is a little above, and has been below 32°, there will frequently be a small button of ice at the foot of the wet-bulb thermometer, which is not easily perceived, and which will keep it at 32° when the temperature of evaporation is really above that point.

2ndly. It is well known that under certain circumstances water may be cooled below 32° without freezing; and an example will perhaps best show the error which this fact may occasion.

Let us suppose that the temperature of the air is 27°, and that when the thermometer is wetted it sinks to 26°, and then rises. Should it rise very slowly, or not at all, the probability is that 26° is the true temperature of evaporation, but if rapidly, the rise may be due to the conversion of the water into ice; and it will be prudent to observe whether or not the thermometer again commences to sink.

We have frequently observed this phenomenon, and I am quite at a loss to what to ascribe its uncertainty.

It has occurred both in a high wind and a calm (the thermometers are protected from the full force of the wind), and it also appeared to be quite uncertain at what temperature the water might freeze.

I am obliged to admit that the limits of certainty of the factors below zero are not quite so close as could be desired. This is partly attributable to our being obliged to reject many observations made with a thermometer which was broken before its index-errors were fully ascertained; but Mr. Campbell and I must claim the indulgence of those who know the difficulty of taking observations requiring so much time and accuracy at such temperatures, and frequently at six o'clock in the morning.

Believe me, &c.,

W. Noble,
Lt. R.N.

C. R. Weld, Esq., Assist. Sec. Royal Soc.

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CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 242.]

Nov. 13, 1854.—A paper, by R. L. Ellis, Esq., was read, entitled "Remarks on the Fundamental Principle of the Theory of Probabilities."

Also, "On the Purbeck Strata of Dorsetshire." By the Rev. O. Fisher.

The object of this paper was to describe the beds from which a series of insect remains and other fossils had been collected by the author, and presented to the Woodwardian Museum.

The connexion of the Purbeck beds with the Oolitic rather than with the Wealden series was maintained, while both were shown to be unconformable in this district to the cretaceous system. Reasons were given for thinking that the materials, of which both the Wealden and Purbeck were composed, had travelled from west to east; and the beds of the New Red Sandstone, as they occur in Devonshire, were pointed out as affording a mass of strata which would furnish a detritus of the character of a large portion of the Hastings sands of Hampshire and Dorsetshire.

In describing the Purbeck beds, the author followed the system of the late Professor E. Forbes, dividing them into upper, middle, and lower; and entered into some detail of the alternations of salt and freshwater conditions that prevailed during their deposition. The aspects under which the same beds appear at different points of the district under examination were particularized, and it was attempted to be shown that these were in conformity with the theory of a current setting from the west towards the east. The mode of occurrence of the remains of insects in the middle and lower Purbecks was somewhat minutely described, and it was suggested that some interesting chronological speculations might be grounded upon it.

The paper concluded with an attempt to explain the singular fractured condition of about thirty feet of the lower Purbeck strata throughout the eastern part of the county. It was supposed that this might have been caused by the deposition of sediment upon the remains of the Portland forest before the mass of the trees had been removed by decomposition; the sediment, after it had become consolidated, settling unequally as the carbonaceous matter was gradually removed.

Nov. 27.—Prof. Willis gave an account of a new form of Atwood's Machine.

Dec. 11.—A communication was made by Dr. Paget on a case of involuntary tendency to fall forwards.

Feb. 19, 1855.—Mr. Hopkins gave a lecture on certain changes of Terrestrial Temperature, and the causes to which they may be attributed.

March 5.—Dr. Clark gave an account of some recent discoveries respecting the origin, migrations, and metamorphoses of Entozoa, and their bearing on the notion of spontaneous generation.

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April 23.—A paper was read by the Master of Trinity, on Plato's Survey of the Sciences, contained in the seventh book of the Republic.

Plato, like Francis Bacon, took a review of the sciences of his time; and like him, complained how little attention was given to the philosophy which they involved. The sciences which Plato enumerates are arithmetic and plane geometry, treated as collections of abstract and permanent truths; solid geometry, which he "notes as deficient" in his time, although, in fact, he and his school were in possession of the doctrine of the "five regular solids;" astronomy, in which he demands a science which should be elevated above the mere knowledge of phænomena. The visible appearances of the heavens only suggest the problems with which true astronomy deals; as beautiful geometrical diagrams do not prove, but only suggest geometrical propositions. Finally, Plato notices the subject of harmonics, in which he requires a science which shall deal with truths more exact than the ear can establish, as in astronomy he requires truths more exact than the eye can assure us of. It was remarked also, that such requirements had led to the progress of science in general, and to such inquiries and discoveries as those of Kepler in particular.

May 21.—A paper was read "On the singular Points of Curves." By Professor De Morgan.

Mr. De Morgan defines a curve as the collection of *all* points whose co-ordinates satisfy a given equation; and contends for this definition as necessary in geometrical *algebra*, whatever limitation may be imposed in algebraic *geometry*. He divides singular points into points of singular *position* and points of singular *curvature*; the character of the former depending on the axes, but not that of the latter. Both species are defined as possessing a notable property, and such as no arc of the curve, however small, can have at all its points.

The form first considered is that of which the case usually taken is an *algebraic* curve. Let $\phi(x, y)$ be a function which for all real and finite values of x and y is real, finite, and *univocal*; let the curve be $\phi(x, y)=0$, considered as an individual of the family $\phi(x, y)=\text{const.}$ The two curves $d\phi : dx=0$, $d\phi : dy=0$, or $\phi_x=0$, $\phi_y=0$, are the *subordinates* of this system, on which the singular points of all depend.

When ϕ is not reducible to another function of the same kind by extraction of a root, it divides the plane of co-ordinates into *regions* in which, severally, it is always positive or always negative. By this consideration it is easily shown (independently of y', y'', &c.), that if $(x+dx, y+dy)$ be a point on the tangent at (x, y), $\phi(x+dx, y+dy)$ has the sign of $\phi_{xx}dx^2+2\phi_{xy}dxdy+\phi_{yy}dy^2$. Hence, immediately after leaving the curve, ϕ agrees with or differs from $-\phi_y y''$ at the point left, according as the curve is left on the convex or the concave side. Hence easily follow the criteria of flexure, and also the following relation between any two points whatsoever of the curve.

Let two points be called *similar* when a line drawn from one to the other cuts the curve an even number of times (0 included) with the same *abutments* (on convexity or on concavity), or an odd number of times with different abutments. Let other points be

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called *dissimilar*. These points are similar or dissimilar, according as their values of $\phi_y \cdot y''$ agree or differ in sign.

An *à priori* proof is given that multiple points, cusps, and isolated points, must be determined by $\phi_x=0$, $\phi_y=0$, or can only take place when both subordinates meet the curve. It is shown that, in the system $\phi(x, y)=$ const., the cusp of $\phi(x, y)=0$ must be an evanescent loop, and the isolated point an evanescent oval, or bounded portion. Some discussion of the meaning of $y'=a+b\sqrt{-1}$ at an isolated point is given.

There have been two methods of treating the singular points. The first has recourse to the theory of equations, using differentiation, if at all, only to supply coefficients. The second attempts canonical forms derived from differential coefficients, and examines, in succession, the meaning and bearing of the successive orders of differential coefficients. Mr. De Morgan affirms that this second method cannot be what it pretends to be; and, by treating it generally, shows that its questions are ultimately dependent upon the theory of equations. An equation of the form $\Sigma A y'^x=0$, when it has no equal roots, decides the character of a singular point definitively; and reduces it to a number of intersecting branches without contact, a number of coinciding isolated points without real tangents, or some of one and some of the other. When the equation has some real roots, each set furnishes either multiple branches with contact, or cusps, or conjugate points with real tangents. All this is easily illustrated by examining the curve in which $\phi(x, y)$ is an infinitely small constant, near to the singular point of $\phi(x, y)=0$.

A theorem given by Lagrange, and strongly indicated in the writings of Newton, Taylor, Stirling, Cramer, and John Stewart, but apparently nearly forgotten, solves the question of finding the higher or lower degrees of all the roots of $\Sigma A y^a=0$, where A is a function of x of the degree a; that is, where $A=x^a(a+A')$, and A' vanishes when $x=\infty$ or when $x=0$. By this theorem (which is also given in the first* Number of the Quarterly Journal of Mathematics), y being $x^r(u+U)$, all the values of r, and their corresponding values of u, are very easily found; and repetition of the process upon a transformed equation gives $U=x^{r_1}(u_1+U_1)$, and so on. It obviously follows, that when the origin is removed to any singular point of a curve, the discussion of the branches which pass through that point, and of their contacts with the tangent and each other, is made very easy. In proof of this, the author takes the following instance,—

$$x^{12}+x^{14}+x^{11}y-x^8y^2+2x^7y^3-x^4y^4+y^6-3xy^3+x^{14}y^{13}=0,$$

and discusses its infinite branches, and the sextuple point at the origin (which turns out to be a couple of isolated points, and a cusp of similar flexures), with very much less space and trouble than ordinary methods would demand from a much less complicated instance. It is also shown that the *lower form* of Lagrange's theorem solves the following question:—Given an equation with a certain number of

* There attributed to Mr. Minding, by a mistake caused by M. Serret, who incorporates it with a theorem of Mr. Minding, without any notice of its author.

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equal roots, what effect will be produced upon these roots by given infinitesimal alterations in the coefficients, how many will remain real, and how many will become imaginary?

Newton has given the foundation and the chief step of a geometrical method (*Newton's parallelogram*) which has passed into oblivion, though it occurs in the celebrated second letter to Oldenburg, has been fully described by Stirling, used by Taylor and De Gua, and forms the main method of Cramer's work on curves. Mr. De Morgan proposes to call it the method of *co-ordinated exponents*.

He proceeds to describe and enlarge this method; observing that, of the polygon which represents an equation, Newton and his followers are in full possession of the connexion of the sides with the solutions, and fail only in not grasping the connexion of the whole polygon with the whole equation. Both Newton's method and Lagrange's, the second of which is an arithmetical version of the first, may be applied to irrational equations, but it will be convenient to confine the description to the form $\Sigma a x^m y^n = 0$, where m and n are integers.

In $ax^m y^n$, let n be an abscissa, and m an ordinate, and let (m, n) be called the exponent point of the term $ax^m y^n$. Take some paper ruled in squares (or ruled both ways in any manner, for any equal rectangles will do) to facilitate the process when n and m are always integers, and lay down all the exponent points in $\Sigma a x^m y^n = 0$. Through some of these points draw a *convex* polygon including all the rest, which can only be done in one way. Should the points be so many and so scattered that some *method* must be applied, the geometrical method is a translation of the main arithmetical method of Lagrange's theorem. The points which end on, or otherwise fall in, the sides of the polygon show the *essential* terms of the equation: no others are wanted to determine q and u in $y = x^r(u + \mathrm{U})$. The upper contour of the polygon shows how all the solutions commence in descending powers of x; the under contour does the same for ascending powers. Take any side of either contour, its projection on the axis of n shows the number of roots it represents, the tangent of the angle it makes with the *negative* side of the axis of n shows the value of r.

It will not be needful to abstract the developments given in the paper: we shall only notice the *inverse* method. The following example is taken, and the construction of the equation is even easier (under Cramer's form) than the direct treatment of it. The example chosen by the author is the following:—Required $\phi(x, y) = 0$, of the twelfth dimension in terms of y, such that the twelve roots of y, with reference to lower degrees, shall be as follows: two roots of the degree 1, four of $\frac{1}{2}$, two of 0, one of -1, two of $-\frac{1}{2}$, one of -2. But with reference to higher degrees, there are to be one root of the degree 3, two of $\frac{1}{3}$, three of 0, three of $-\frac{1}{3}$, two of -1, one of -3. On examination these conditions are found compatible, and the most general equation which satisfies the conditions is found.

The paper is terminated by a discussion on the pointed branch, for the admission of which, as a branch altogether composed of singular points, the author contends.

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GEOLOGICAL SOCIETY.

[Continued from p. 240.]

February 20, 1856.—D. Sharpe, Esq., President, in the Chair.

The following communications were read :—

1. "Notice of a Visit to the Dead Sea." By H. Poole, Esq. Forwarded from the Foreign Office by order of Lord Clarendon.

Mr. Poole went to this district to look for nitre, which was reported to occur there; but he met with none, and found reason to suppose that the report was unfounded. He noticed bituminous shales at Nebi Mousa, and sulphurous earths both there and at El Lisan on the Dead Sea, but the sulphur was not found in any large quantity. The author exhibited to the meeting a series of these deposits, and of rock-salt and other minerals from the neighbourhood of the Dead Sea, together with recent natural history specimens, volcanic and other rock-specimens, and some tertiary and cretaceous fossils from the district visited.

2. "On the Affinities of the great extinct Bird (*Gastornis parisiensis*, Hébert) from the lower Eocene near Paris." By Prof. Owen, F.R.S., F.G.S.

Prof. Owen communicated the results of his comparisons of the fossil tibia of the *Gastornis parisiensis*, Hébert,—a large bird from the lower Eocene deposits at Meudon near Paris—with the tibiæ of known recent and fossil birds.

The tibia of the *Gastornis* presents the same median position of the supra-tendinal bridge as in the Albatross and the lamellirostral web-footed birds; but, as the same position of the bridge occurs in the *Notornis*, the Gallinule, the Raven, and some accipitrine birds, that character is not conclusive of the affinities of the *Gastornis* to the Palmipeds; and it is further invalidated by a difference in the aspect of the plane of the lower outlet of the bridge. In the Albatross (*Diomedæa*) and the Lamellirostres, the foramen or outlet looks directly forwards; its plane is vertical. In the oblique aspect of that outlet, the *Gastornis* more resembles the large Waders (*Grallæ*) and the *Dinornis* tribe. Amongst the *Gallinaceæ*, the Turkey (*Meleagris*) nearly resembles the *Gastornis* in the position of the bridge; and more nearly resembles it than does the Albatross or the Swan in the low tuberosity external to the bridge above the base of the outer condyle, as well as in the shallow groove dividing that tuberosity from the bridge. The depression on the fore-part of the tibia above the distal condyles, if natural to the *Gastornis*, is a structure not precisely repeated in any of the *Grallæ*. In the *Ciconia Argala* the anterior interspace of the condyles forms a cavity, bounded above by the tubercle and ridge developed from the bridge, and by the oblique converging upper borders of the condyles below. The canal of the bridge opens below into the concavity. In the *Grus Antigone* the lower border of the outlet of the bridge defines, with a tubercle external to it, the shallow supracondyloid cavity; but there is no definite fossa, like that in the *Gastornis*.

In the *Notornis*, the breadth of the lower end of the tibia a little

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exceeds the depth or fore-and-aft diameter of the condyles. The supra-tendinal bridge is of moderate breadth, is transverse, and median in position; its lower outlet looks forward just above the wide and shallow intercondyloid space. The extinct *Aptornis* chiefly differs from the *Notornis* in the less median position of the bridge, and in the more shallow canal leading to it. In the *Dinornis*, the breadth and depth of the condyles are equal; the outer condyle is the broadest, the inner one is the most prominent; their articular surfaces are so continuous as to leave no space answering to the intercondyloid space in the *Aptornis*, *Notornis*, &c. The bridge is situated nearer the inner side of the bone, is subtransverse, rather narrow, with a widely elliptical lower outlet opening above the inner condyle.

The *Gastornis* was a bird of the size of the Ostrich, but with more bulky proportions, and in that respect more resembling the *Dinornis*: it appears to have had nearer affinities with the wading order, and therein, perhaps, to the *Rallidæ*; but the modifications of its tibia indicate a genus of birds distinct from all previously known genera.

3. "Description of some Mammalian Fossils from the Red Crag of Suffolk." By Prof. Owen, F.R.S., F.G.S.

The fossils described in this paper were referred by the author to the following genera and species:—*Rhinoceros*, a species nearly allied to, if not identical with, *Rh. Schleiermacheri*, Kaup; from crag-pits at Wolverston, Sutton, and Felixstow, Suffolk. *Tapirus priscus*, Kaup; from Sutton. *Sus palæochœrus*, Kaup; from Sutton. *Sus antiquus*, Kaup; from Ramsholt, Suffolk. *Equus*: two species, one apparently *Eq. plicidens*, Owen; from Bawdsey, Suffolk. *Cervus dicranocerus*, Kaup; from Ipswich and Sutton. *Cervus megaceros*, from Felixstow. *Ursus*, sp. indet., less than *Ur. spelæus*. *Canis*, apparently *C. Lupus*. *Felix pardoïdes*, Owen; from Newbourn, Suffolk. *Mastodon longirostris*, Kaup; from Sutton, Felixstow, and Ipswich. *Ziphius longirostris*, Cuv. (*Dioplodon Becanii*, Gervais); *Hoplocetus crassidens*, Gervais; *Balænodon affinis*, *Bal. definita*, *Bal. gibbosa*, *Bal. emarginata*, Owen; and remains of species of *Delphinus*, of the size of the Grampus.

The conclusion which the author deduced from the large proportion of miocene forms of mammalia, and the very great numerical superiority of individual fossil specimens from the Red Crag referable to miocene species, and from the admixture of these fossils with a few eocene and pleistocene species, was that the Red Crag was the *débris* of former tertiary strata of different periods, and, in a great proportion, of the miocene period.

March 5, 1856.—D. Sharpe, Esq., President, in the Chair.

The following communications were read:—

1. "Notes on the Geology of some parts of South Africa." By R. N. Rubidge, Esq. In a letter to Sir Roderick Murchison, F.G.S.

Mr. Rubidge first referred to the occurrence of gold at Smithfield in the Orange River Sovereignty, as detailed in his letter of May 1854, published in the Society's Journal, No. 41; and stated that several pieces of gold had since been found at the spot described in

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the letter referred to. Besides being found in the alluvium there, gold was met with in a quartz-vein in the trap traversing the stratified rock,—in other quartz associated with the trap,—and in a mass of limestone enclosed in the trap-dyke;—but none in the stratified rock itself (which belongs to the Dicynodon or Karoo Series).

Mr. Rubidge next alluded to the fossil plants which he there found in the strata; some of these he referred with doubt to *Calamites*. Six years ago also the author found numerous vegetable remains (some of which were possibly referable to *Lepidodendron*) at Jackall's Kop, on the eastern side of the Stormberg Range, in the same formation as that of the Drakensberg and Smithfield; and Calamite-like plants in the western part of the Zuurbergen. The author remarked that the plant-remains above referred to much resembled those collected by Mr. Bain at the Ecca Heights in rocks of the Karoo Series. Mr. Rubidge had also found bones of the Dicynodon near the Caledon River and at Halse's farm six miles from Smithfield.

From various observations by himself and others, the author had been enabled to recognize the existence of the Dicynodon or Karoo rocks in the Drakensberg, at Harriesmith, at Winburg, and even at Megaliesberg: and Dr. Sutherland has lately described the same rocks in Natal, where they are rich in coal.

The amygdaloid rock which supplies the agate-gravel of the Orange, Caledon, Kroai, and Vaal Rivers appears to exist in the "Mont des Sources" in the Drakensberg, as an unworn specimen was found in the Eland River (a tributary of the Vaal), not more than twelve miles from its source.

Lastly, Mr. Rubidge supplied some remarks on the geology of the copper district of Namaqualand and bordering countries. Granitic rocks of several varieties occur, together with gneiss, mica-schist, and talc-schist. The gneiss strikes 5° to 20° S. of W., and dips alternately N. and S.; one dip continuing for many miles. On the hills the gneiss and schists are covered by horizontal sandstones, which appear to be the same as the sandstone of Table Mountain, and continuous with it.

The copper is found in fissures of the gneiss, where it is locally disturbed in its dip, the strike remaining unaltered; that is, along anticlinal and synclinal folds or axes; also in fissures extending nearly in the direction of the magnetic meridian, and in crevices between masses of rock, with no veinstone or gangue: the oxides and silicates often appear to be infiltered into the rock-mass. The ores most common are red and black oxides, green and blue silicates, purple and yellow sulphurets, and a few carbonates. Granitic rocks are often found in the axes above referred to.

2. "On the Lowest Sedimentary Rocks of the South of Scotland." By Prof. Harkness, F.G.S.

The author first described in detail the indications of the axis of the Silurian rocks of Dumfriesshire. It is well seen on the Esk River and the Rennel Burn running into the Esk,—on the Dryfe water a little above Borland Bridge,—in the Shaw Burn, and in the upper part of Auchenrodden Burn in Applegarth parish. Here it is inter-

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rupted by the Corncockle Sandstone, but reappears, or its proximity is traceable, in Lochmaben and Torthorwald parishes, striking towards the Criffel in Kirkcudbrightshire. This E.N.E. and W.S.W. direction agrees with that of the axis observed by Mr. Nichol in Roxburghshire. The axis itself, where seen (in the Rennel Burn, in Dryfe water, and in Torthorwald), consists of an anticlinal of purple grits. The overlying rocks are thin-bedded and alternating sandstones and purplish shales; and these appear to have been much folded, repeated again and again by flexures, and considerably altered by the consequent pressure.

The author then adduces evidence of these sandstones and shales having been deposited in shallow water, and probably under littoral conditions. The sandstones to the south of the axis, at Binks in Roxburghshire, are ripple-marked, and the alternations of sandstone and shale are frequent. There have been here observed casts of desiccation-cracks,—surface-pits, resulting, in the author's opinion, from littoral action,—Annelid tracks,—the track of a small animal, probably Crustacean, resembling in miniature the *Protichnites* of the Potsdam sandstone,—and Fucoids.

In the low-lying purplish shale (overlying the axis to the south) which occurs with thin-bedded sandstone at the Upper Cleugh Burn, in Applegarth parish, *Protovirgularia* is met with, which is found also in the Barlae flags; and *Graptolites* occur at Dalton Rocks, in the parish of Dalton (also on the south side of the axis), in shale associated with ripple-marked sandstone, and distinct from the other Graptolitic rocks of Dumfriesshire.

On the south side of the axis there is no trace of the great Anthracitic and Graptolitic bands which traverse the north-eastern part of Dumfriesshire; and the author thinks that no strata occupying so high a position are developed on the south side of the axis in the district under notice. He thinks also that the fossiliferous beds of Grieston (Peeblesshire) and what he regards as their equivalents in Kirkcudbrightshire (the Barlae flags) should have a lower position than that of the anthracitic and graptolitic shales assigned to them; although, from the numerous flexures that the rocks have here undergone, their relative positions is much obscured; and he thinks that they may be the Scottish representatives of the fucoidal sandstones underlying the Graptolite-beds of Sweden and Norway.

Mr. Harkness regards the fossiliferous shales and sandstones, more particularly referred to in this communication, as underlying the Barlae and Grieston flags, and as the lowest rocks in Scotland that have yet afforded fossils; and therefore as containing some of the earliest records we possess of organized existence.

3. "On Fossil Remains in the Cambrian Rocks of the Longmynd." By J. W. Salter, Esq., F.G.S.

In this paper the author communicated the discovery of organic remains in some of those ancient sediments which have hitherto been termed "Azoic." Of these fossils, some (traces of Annelides and fragments of a Trilobite) were found by Mr. Salter in the unaltered sandstone beds on the eastern side of the Longmynd; and another (a Fucoid?) he discovered in the Moel-y-ci near Bangor.

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The rocks of the Longmynd that have yielded the fossils referred to are nearly vertrical beds of hard flaggy sandstone, coinciding in strike with that of the Longmynd, and about 1½ mile east of the principal ridge. These beds form part of a series of bluish-grey sandstones, alternating with purplish slaty beds, all of which lie *below* the conglomerates and red sandstones of the Portway, and *above* the thick series of dark-olive schists, seen at Church Stretton, &c., which are the lowest portion of the Longmynd series.

Of the Annelid traces, some (which the author has referred to *Arenicola didyma*) were found at Stretton, Callow Hill, and other spots in the upper portion of the sandstone above mentioned, where it is flaggy, rippled, and micaceous. Annelid tubes or tracks were also found at Callow Hill in the same rock. The most interesting of the fossils from this sandstone, however, are the indications of fragments, cephalic (?) and caudal, of a trilobite allied apparently to the *Deikelocephalus* of Dr. Owen. To the Longmynd Trilobite Mr. Salter has given the name of *Palæopyge Ramsayi*. It occurred at Little Stretton, &c. The author also described in detail some of the surface-markings of the flags, which he referred to ripples and littoral action.

ROYAL INSTITUTION OF GREAT BRITAIN.

January 25, 1856.—"Inferences from the Negation of Perpetual Motion." By W. R. Grove, Esq., Q.C., F.R.S., M.R.I.

Scattered among the writings of philosophers will be found allusions to the subject of perpetual motion, and here and there are arguments like the following; such a phænomenon cannot take place, or such a theory must be fallacious, because it involves the idea of perpetual motion: thus Dr. Roget advanced as an argument against the contact theory of electricity, as originally propounded, that if mere contact of dissimilar metals, without any chemical or molecular change, could produce electricity, then as electricity could in its turn be made to produce motion, we should thus get perpetual motion.

It may be well to define, as far as such a definition is possible, what is commonly meant by the term perpetual motion. In one sense, all motion, or rather all force, is perpetual; for example, if a clock weight be wound up, it represents the force derived from the muscles of the arm which turns the key; the muscles again derive force indirectly from the chemical action of the food, and so on. As the weight descends, it conveys motion to the wheels and pendulum; the former giving force off in the form of heat from friction, the latter communicating motion to the air in contact with it, thence to the case of the clock, thence to the air of the room,—proved in a very simple manner by the ticking heard, which is, in fact, a blow to the organ of hearing. Although ultimately lost to our senses, there is no reason to suppose that the force is ever in fact lost. The weight thus acting, reaches the ground quietly, and produces no effect at the termination of its course.

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If, instead of being allowed to communicate its force to the works of the clock, the weight be allowed to descend suddenly, as by cutting the string by which it is suspended, it strikes the floor with a force which shakes the house; and thus conveys, almost instantaneously, the amount of force which would be gradually dissipated, though not ultimately consumed, by the clock in a week or nine days.

This idea, however, of the perpetuity of force, is not what is commonly understood by the term perpetual motion: that expression is used to convey the notion of a motive machine, the initial force of which is restored by the motion produced by itself,—a clock, so to speak, which winds itself up by its own wheels and pendulum, a pump which keeps itself going by the weight of the water which it has raised. Another notion, arising from a confusion between static and dynamic forces, was, that motion might be obtained without transferring force, as by a permanent magnet. All sound philosophers are of opinion that such effects are impossible; the work done by a given force, even assuming there were no such thing as friction, aërial resistance, &c., could never be more than equal to the initial force; the theoretical limit is equilibrium. The weight raised at one end of a lever can never, without the fresh application of extraneous force, raise the opposite weight which has produced its own elevation. A force can only produce motion when the resistance to it is less powerful than itself; if equal, it is equilibrium: thus if motion be produced, the resistance, being less than the initial or producing force, cannot reproduce this; for then the weaker would conquer the stronger force.

The object of this evening's communication was not, however, to adduce proofs that perpetual motion, in the sense above defined, is impossible; but assuming that as a recognized truth, to show certain consequences which had resulted, and others which were likely to result, from the negation of perpetual motion; and how this negation may be made a substantive and valuable aid to scientific investigation.

After Œrsted made his discovery of electro-magnetism, philosophers of the highest attainments argued, that as a current of electricity, circulating in a wire round a bar of iron, produced magnetism, and as action and reaction are equal, and in contrary directions, a magnet placed within a spiral of wire should produce in the wire an electrical current: had it occurred to their minds that, if a permanent magnet could so produce electricity, and thence necessarily motion, they would thus get, in effect, perpetual motion, they would probably have anticipated the discovery of Faraday, and found that all that was required was to move the magnet with reference to the wire, and thus electricity might have been expected to be produced by a magnet without involving the supposed absurdity.

In a very different instance, viz. the expansion of water when freezing, not only heat, or the expansive force given to other bodies by a body cooling, would be given out by water freezing, but also the force due to the converse expansion in the body itself; and upon the argument that force would, in this case, be got out of nothing,

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Mr. J. Thomson saw that this supposed impossibility would not result if the freezing-point of water were lowered by pressure, which was experimentally proved to be the case by his brother.

In the effects of dilatation and contraction by heat and cold, when applied to produce mechanical effects, and consequently in the theory of the steam-engine, this subject possesses a greater practical interest. Watt supposed, that a given weight of water required the same quantity of what is termed total heat (that is, the sensible added to the latent heat) to keep it in the state of vapour, whatever was the pressure to which it was subjected, and consequently, however its expansive force varied. Clement Desormes was also supposed to have experimentally verified this law. If this were so, vapour raising a piston with a weight attached would produce mechanical power; and yet the same heat existing as at first, there would be no expenditure of the initial force; and if we suppose that the heat in the condenser was the real representative of the original heat, we should get perpetual motion. Southern supposed that the latent heat was constant, and that the heat of vapour under pressure increased as the sensible heat. M. Despretz, in 1832, made some experiments which led him to the conclusion that the increase was not in the same ratio as the sensible heat, but that yet there was an increase; a result confirmed and verified with great accuracy by M. Regnault, in some recent and elaborate researches. What seems to have occasioned the error in Watt and Clement Desormes' experiments was, the idea involved in the term latent heat; by which, supposing the phænomenon of the disappearance of sensible heat to be due to the absorption of a material substance, that substance, 'caloric,' was thought to be restored when the vapour was condensed by water, even though the water was not subjected to pressure; but to estimate the total heat of vapour under pressure, the vapour should be condensed while subjected to the same pressure as that under which it is generated, as was done in M. Despretz and M. Regnault's experiments.

Carnot's theory, that the mechanical force is produced by the transfer of heat, and that there is no ultimate cost or expenditure of heat in producing it, was founded in part on similar considerations; it is true that mechanical *motion* may be produced by the transfer of heat from a higher to a lower temperature, without ultimate loss, or, strictly speaking, with an infinitely small loss, but not, as he seemed to think, an available mechanical force, except upon an assumption which he did not make, and to which allusion will presently be made. Thus, let a weight be supposed to rest on a piston confining air of a certain temperature, say 50°, in a vessel non-conducting for heat; part of this temperature will be due to the pressure exerted, since compression produces heat in air, while dilatation produces cold. If the air be now heated, say to 70°, the piston, with the weight attached, will rise, and the temperature in consequence of the expansion of the air will cool somewhat, say to 69° (the heat of friction of the piston may be taken to compensate the power lost by friction): if now a cold body be made to abstract 20°, the piston descending will, by its pressure, restore the 1° lost by expansion;

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and when the piston has returned to its first position, the original 50° will remain as at first. Suppose this experiment repeated up to the rise of the piston, but when the piston is at its full elevation, and the cold body is applied, let the weight be removed, so as to drop upon a wheel, or to be used for other mechanical purposes, the descending piston will not now reach its original point without more heat being abstracted; from the removal of the weight there will not be the same force to restore the 1°, and the temperature will be 49°, or some fraction short of the original 50°; if this were otherwise, then as the ball in falling may be made to produce heat by friction, we should have more heat than at first, or a creation of heat out of nothing; in other words, perpetual motion. When force is abstracted from a thermal machine we ought to lose heat.

If we suppose that the degrees of heat at the lower temperature represent the same amount of force as the same number of degrees at the higher temperature; if, for instance, we suppose that a body cooling from 120° to 100°, gives off the same force as a body cooling from 20° to zero; this seems to be tacitly assumed by Carnot, but is probably not correct, the results of high-pressure steam and other facts indicating a contrary conclusion. If then the 20° on the lower scale do not represent an equivalent force to the 20° on the higher, we *may* gain the same heat in degrees in the condenser as was lost from the furnace, and yet get derived power. There is frequently a confusion between the work performed which returns to the machine, and the derived work, or that which does not return, and is used for other purposes. This is puzzling to the reader of treatises on the steam-engine, and kindred subjects, and has led to much obscurity of thought and expression.

M. Seguin, in 1839, contraverted the position that derived power could be got by the mere transfer of heat, and by calculation from certain known data, such as the law of Mariotte, viz. that the elastic force of gases and vapours increased directly with the pressure, and assuming that for vapour between 100° and 150° Centigrade each degree of elevation of temperature was produced by a thermal unit, he deduced the equivalent of mechanical work capable of being performed by a given decrement of heat; and thus concluded that for ordinary pressures about one gramme of water losing one degree Centigrade would produce a force capable of raising a weight of 500 grammes through a space of one metre; this estimate is a little beyond that given by the more recent experiments of Mr. Joule. M. Seguin has, however, since the accurate and elaborate experiments of M. Regnault, necessarily varied his estimate, as by these experiments it appears that, within certain limits, for elevating the temperature of compressed vapour by one degree, no more than about $\frac{3}{10}$ths of a degree of total heat is required; consequently, the equivalent multiplied in this ratio would be 1666 grammes, instead of 500. Other investigators have given numbers more or less discordant, so that without giving any opinion on their different results, this question may be considered at present far from settled. M. Regnault himself does not give the law by which the ratio of

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heat varies with reference to the pressure, and is still believed to be engaged in researches on the subject, one involving questions of which experiments on the mechanical effects of elastic fluids seem to offer the most promising means of solution.

One of the greatest difficulties which had presented itself to Mr. Grove's mind, with reference to the theory of Carnot, had been one of analogy, derived from the received theories of electricity. Many electrical cases might be cited in which no electricity is supposed to be lost, though a certain mechanical effort is produced by the electricity; if, for instance, a ball vibrates between a positively and a negatively electrified substance, none of our electrical theories lead us to believe that any difference in the actual amount of electricity transferred would be occasioned by the ball being attached to a lever which would strike a wheel or produce any other mechanical effect.

In preparing this evening's communication an experiment had occurred to him, which, though performed with imperfect apparatus and therefore requiring verification, does, as far as it goes, support the view derived from the negation of perpetual motion, viz. that when electricity performs any mechanical work which does not return to the machine, electrical power is lost. The experiment is made in the following manner. A Leyden jar of one square foot coated surface has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about half an inch apart). Between the Leyden jar and the prime conductor is inserted a small unit-jar of nine square inches surface, the knobs of which are 0·2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between the attracting knobs, and the Leyden jar charged by discharges from the unit-jar. After a certain number of these (twenty-two in the experiment performed in the theatre on this occasion), the discharge of the large jar takes place across the half-inch interval; this may be viewed as the expression of electrical power received from the unit-jar. The experiment is now repeated, the wire between the balls having been removed, and therefore the 'tip' or the raising of the weight is performed by the electrical repulsion and attraction of two pairs of balls; at twenty-two discharges of the unit-jar the balance is subverted, and one knob drops upon the other, but *no discharge takes place*, showing that some electricity has been lost, or converted into the mechanical power which raises the balance. By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and would be restored if the ball were brought back, without discharge, by extraneous force.

This experiment has succeeded in so large an average of cases, and so responds to theory, that, notwithstanding the imperfection of the apparatus, Mr. Grove places much reliance on it; indeed, it is difficult to see, if the discharges or other electrical effects were the same in both cases, why the raising the ball, being extra and the ball

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being capable by its fall of producing electricity or other force, force would not thus be got out of nothing, or perpetual motion attained.

The experiment is believed to be new, and to be suggestive of others of a similar character, which may be indefinitely varied*. Thus, two balls made to diverge by electricity should not give to an electrometer the same amount of electricity as if they were, whilst electrified, kept forcibly together, an experiment which may be tried by Coulomb's torsion balance.

There is an advantage in electrical experiments of this class, as compared with those on heat, viz. that though there is no perfect insulation for electricity, yet our means of insulation are immeasurably superior to any attainable for heat.

Similar reasoning might be applied to other forces; and many cases bearing on this subject, have been considered by Mr. Grove in his essay on the "Correlation of Physical Forces."

Certain objections to these views were then discussed, and especially some apparently formidable ones presented by M. Matteucci in a paper published by him some time ago†.

This distinguished philosopher cites the fact, that a voltaic battery decomposing water in a voltameter, while the same current is employed at the same time to make an electro-magnet, nevertheless gives in the voltameter an equivalent of gas, or decomposed substance, for each equivalent of chemical decomposition in the cells, and will give the same ratios if the electro-magnet be removed. In answer to this objection it may be said, that in the circumstances under which this experiment is ordinarily performed, several cells of the battery are used, and so there is a far greater amount of force generated in the cells than is indicated by the effect in the voltameter. If, moreover, the magnet is not interposed, still the magnetic force is equally existent through the whole circuit; for instance, the wires joining the plates will attract iron filings, deflect magnetic needles, &c. By the iron core a small portion of the force is absorbed while it is being made a magnet, but this ceases to be absorbed when the magnet is made; this is proved by the recent observations of Mr. Latimer Clarke, which were fully entered into and extended by Mr. Faraday, in a lecture at the Institution (Jan. 20, 1854‡). It is like the case of a pulley and weight, which latter exhausts force while it is being raised, but when raised the force is free, and may be used for other purposes.

If a battery of one cell, just capable of decomposing water and no more, be employed, this will cease to decompose while making a magnet. There must, in every case, be preponderating chemical affinity in the battery cells, either by the nature of its elements or by the reduplication of series, to effect decomposition in the voltameter, and if the point is just reached at which this is effected, and the power is then reduced by any resistance, decomposition ceases: were it otherwise, were the decomposition in the voltameter

* See some observations on this experiment in our last Number, p. 225.

† *Archives des Sciences Physiques*, vol. iv. p. 380.

‡ Phil. Mag. vol. vii. p. 197.

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the exponent of the entire force of the generating cells, and these could independently produce magnetic force, this latter force would be got from nothing, and perpetual motion be obtained.

In another case, cited by M. Matteucci, viz. that a piece of zinc dissolved in dilute sulphuric acid gives somewhat less heat than when the zinc has a wire of platinum attached to it, and is dissolved by the same quantity of acid, the argument is deduced, that as there is more electricity in the second than in the first case, there should be less heat; but, as according to our received theories the heat is a product of the electric current, and in consequence of the impurity of zinc, electricity is generated in the first case molecularly in what is called local action, though not thrown into a general direction, there should be more of both heat and electricity in the second than in the first case; as the heat and electricity due to the voltaic combination of zinc and platinum are added to that excited on the surface of the zinc, and the zinc should be, as in fact it is, more rapidly dissolved. Other instances are given by M. Matteucci, and many additional cases of a similar description might be suggested. But although it is difficult, perhaps impossible, to restrict the action of any one force to the production of one other force, and one only; yet if the whole of one force, say chemical action, be supposed to be employed in producing its full equivalent of another force, say heat; then as this heat is capable in its turn of reproducing chemical action, and, in the limit, a quantity equal or at least only infinitely short of the initial force; if this could at the same time produce independently another force, say magnetism; we could, by adding this to the total heat, get more than the original chemical action, and thus create force or obtain perpetual motion.

The impossibility of perpetual motion thus becomes a valuable test of the approach that in any experiment we may have made to eliminating the whole power which a given natural force is capable of producing; it also serves, when any new natural phænomenon is discovered, to enable us to ascertain how far this can be brought into relation with those previously known. Thus when Moser discovered that dissimilar metals would impress each other respectively with a faint image of their superficial inequalities; that, for instance, a copper coin placed on a polished silver plate, even in the dark, would, after a short time, leave on the silver plate an impression of its own device, it occurred to Mr. Grove, that as this experiment showed a physical radiation taking place between the metals, it would afford a reason for the effects produced in Volta's contact experiment, without supposing a force without consumption or change in the matter evolving it. This led him to try the effect of closely approximating discs of zinc and copper without bringing them into metallic contact; and it was found that discs thus approximated, and then quickly separated, affected the electroscope just as though they had been brought into contact. Without giving any opinion as to what may be the nature of the radiation in Moser's phænomena, this experiment removes the difficulty presented by that of Volta to the chemical theory of electricity.

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The general scope of the argument from the negation of perpetual motion leads the mind to regard the so-called imponderables as modes of motion, and not as different kinds or species of matter; the recent progress of science is continually tending to get rid of the hypotheses of fluids, of occult qualities, or latent entities, which might have been necessary in an earlier stage of scientific inquiry, and from which it is now extremely difficult to emancipate the mind: but if we can, as it is to be hoped we shall, ultimately arrive at a general dynamic theory, by which the known laws of motion of masses can be applied to molecules, or the minute structural parts of matter, it seems scarcely conceivable that the mind of man can further simplify the means of comprehending natural phænomena.

February 22, 1856.—"On certain Magnetic Actions and Affections." By Michael Faraday, D.C.L., F.R.S.

All bodies subject to magnetic induction, when placed in the ordinary magnetic field between the poles of a magnet, are affected; paramagnetic bodies tend to pass bodily from weaker to stronger places of force, and diamagnetic bodies from stronger to weaker places of force. If the bodies are elongated, then those that are paramagnetic set along the lines of force, and those that are diamagnetic across them: but if these bodies have a spherical form, are amorphous, and are perfectly free from permanent magnetic charge, they have no tendency to set in a particular direction. Nevertheless, there are bodies of both classes, which being *crystalline*, have the power of setting when a single crystal is wrought into the form of a sphere, and these are called magnecrystals; their number is increasing continually; carbonate of lime, bismuth, tourmaline, &c., are of this nature. Bodies which being magnetic, set, because they are elongated, are greatly influenced in the force of the set by the nature of the medium surrounding them, and to such an extent that they not merely vary in their force from the maximum to nothing, but will often set axially in one medium, and equatorially in another. Yet the same bodies, if magnecrystallic and formed into spheres, though they set well in the magnetic field, will set with the *same* force whatever the change in the media about them, and are perfectly freed from the influence of the latter. Thus, if a crystal of bismuth formed into a sphere, or a vertical cylinder, has, when suspended, its magnecrystallic axis horizontal, and if the various media about it, from saturated solution of sulphate of iron, up to phosphorus, through air, water, alcohol, oil, be changed one for another, no alteration in the amount of torsion force required to displace the magnecrystal will occur, provided the force of the magnet be constant, notwithstanding that the list of media includes highly paramagnetic and diamagnetic bodies; and in such cases the measurement of the power of set is relieved from a multitude of interfering circumstances existing in other cases, and that power which is dependent upon the internal structure and condition of the substance is proved to be, at the same temperature, always the same.

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A consequence of magnecrystallic structure is that the same body is more paramagnetic, or more diamagnetic in one direction than in another; and therefore it follows, that though such a crystal may have no variation in set-force, produced by change of the surrounding medium, it may have a variation produced in the absolute force of attraction or repulsion; even up to the point of being attracted in one position and repelled in another, though no change in form, or in the surrounding medium, or in the force of the magnet, or in the nature of the body itself, be made, but simply a change in the *direction of the structure*. This was shown by a crystal of the red ferroprussiate of potassa, which, being coated carefully with wax, was suspended from the arm of a torsion balance so that it dipped into a solution of protosulphate of iron occupying the magnetic field*. When the magnecrystallic axis was parallel to the lines of force the crystal was attracted by the magnetic pole, when it was perpendicular to the lines of force the crystal was repelled; acting like a paramagnetic and a diamagnetic in turns. No magnecrystal has yet been found having such a relation to a vacuum, or to carbonic acid (its magnetic equivalent); calcareous spar is nearly coincident with such a medium, and shows different degrees of force in the two directions, but is always a little on the diamagnetic side. Calcareous spar having a trace of iron has been found very nearly up to the desired point, on the paramagnetic side; and as these preserve the full magnecrystallic relation of the two directions, there is no reason to suppose that a crystal may not be found which may not be paramagnetic in one direction, and diamagnetic in another, in respect of space as zero.

There is every reason to believe that the general magnetic relations of a magnecrystal are the same with those of the same substance in the amorphous state; and that the circumstances which influence one, influence the other to the same degree. In that case, the magnetic affections of a body might be ascertained by the examination of the magnecrystallic affections; thus the effect of heat upon bismuth, tourmaline, &c., might be examined by the set of the crystals; and with so much the greater advantage, that short globular forms could be used, perfectly free from the magnetic influence of the surrounding media required as temperature baths, and requiring no displacement of these media with the motion of the crystal. So crystals of bismuth, tourmaline, carbonate of iron, and other bodies were suspended in baths of oil, water, &c., the temperature gradually raised and lowered, and the torsion force of the set for each temperature observed. With bismuth, a crystal having a force of 200 at 20° F. was reduced to a force of 70 at 300°, and the diminution of force appeared to be nearly equal in all parts of the scale for an equal number of degrees. A piece of amorphous bismuth, compressed in one direction, gave nearly the same amount and degree of change for the same alteration of temperature; leading us to the persuasion that the whole magnetic force of bismuth as a diamagnetic body would suffer like change. A crystal of tourmaline, which

* 2½ volumes of saturated solution, at 65° F., and 1 volume of water.

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at 0° had a setting force of 540, when raised to 300°, had a setting force of only 270: the loss of force was progressive, being greater at lower than at high temperatures; for a change from 0° to 30° caused a loss of force equal 50, whilst a change from 270° to 300° caused a loss of only 20. Carbonate of iron suffered a like change; at 0° the force was 1140, at 300° it was only 415; at the lower temperature the loss for 30° was 120 of force, at the upper it was only 34.

In all these and in many other cases, both with paramagnetic and diamagnetic bodies, the magnecrystallic differences diminished with the elevation of temperature; and therefore it may be considered probable, that the actual magnetic force changed in the same direction. But on extending the results to iron, nickel, and cobalt, employing these metals as very small prisms associated with copper cubes to give them weight, it was found that another result occurred. Iron, whether at the temperature of 30° or 300°, or any intermediate degree, underwent no change of force; it remained at 300, which was the expression for the piece employed under the circumstances. We know that at higher temperatures it loses power, and that at a bright red it is almost destitute of inductive magnetic force. A piece of nickel, which at 95° had a setting power of 300, when raised to 285°, had a power of only 290, so that it had lost a thirtieth part of its force; at the heat of boiling oil, it is known to lose nearly all its force, being unable then to affect a magnetic needle. Cobalt, on the other hand, requires a far higher temperature than iron to remove its magnetic character, a heat near that of melting copper being necessary. As to lower temperatures, it was found that an elevation from 70° to 300° caused an absolute *increase* of the magnetic force from 293 to 333. It is evident, therefore, that there is a certain temperature, or range of temperature above 300°, at which the magnetic force of cobalt is a maximum; and that elevation above, or depression below that temperature causes a diminution of the force. The case is probably the same for *iron*; its maximum magnetic force occurring at temperatures between 0° and 300°. If nickel is subject to the same conditions of a maximum, then that state must come on at temperatures below 0°: and it may be further remarked, that as the maximum conditions occur in the following order for ascending temperatures, nickel, iron, cobalt, such also is the same order for the temperatures at which they lose their high and distinctive magnetic place amongst metals.

XL. *Intelligence and Miscellaneous Articles.*

CONTRIBUTION TO THE KNOWLEDGE OF FLUORESCENCE.

BY G. OSANN.

1. I HAVE succeeded in obtaining a fluid which is one of the best of fluorescent liquids, and which may be prepared with great facility and at a very small cost. Our common lampblack is well known to contain a resin which may be extracted from it by means of alcohol. Alcohol, of spec. grav. 0·853, is poured over the lampblack and left standing upon it for about a day, when a yellowish-brown fluid is

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obtained which possesses the property of fluorescence. In this state the fluid is rather too concentrated, and in order to obtain the proper degree of dilution I proceed in the following manner:—I take a quadrangular glass vessel with parallel walls of about an inch and a half in height, half-fill it with alcohol of the above strength, and then add the fluid. By producing a cone of light in this by means of a biconvex glass of short focus, the intensity of the colour will soon show whether the right degree of dilution has been attained. The fluorescence is greenish-blue, like that which is obtained by means of an extract of the seeds of the thorn-apple. Its behaviour towards coloured glasses is also the same. If a brownish-yellow glass be held between the eye and the fluorescent cone of light, the latter is seen almost unaltered; but it disappears almost entirely when the glass disc is brought between the lens and the fluid.

2. *Relation of fluorescence to the electric light.*—The luminous phænomena of electricity may evidently be divided into two classes; namely the phænomena of the electric spark, and the luminosity produced by the ignition of the bodies through which electricity passes. The former light may be produced by the sparks of the machine, or still better, because stronger, by the induction apparatus with Neef's arrangement. I have therefore operated with the latter apparatus. The induction apparatus is connected with an electrometer in such a way, that the wire which touches the lead forms the negative electrode. When the apparatus is in action, a blue light is observed, covering the surface of the wire like a cloak.

The following fluids were poured into test-tubes to a certain height; these were held at the height of the fluids to the electric light, and looked into from above:—1. A solution of sulphate of quinine in water. 2. A decoction of the bark of the horse-chestnut. 3. An alcoholic extract of the seeds of the thorn-apple. 4. A similar extract of turmeric root. 5. An alcoholic extract of litmus (dispersed yellow light). 6. A solution of chlorophyll in alcohol. The result of these experiments was, that the first five fluids were fluorescent, but no fluorescence could be detected in the sixth. A repetition of this experiment gave the same results. From this it would appear that the electrical light is deficient in the rays which produce red in the solution of chlorophyll.

I now instituted a series of experiments in order to ascertain the effects of the light of a platinum wire ignited by the passage of an electrical current. For this purpose a platinum wire of an inch and a half long was fixed in such a manner, that porcelain saucers containing the above-mentioned fluids might be placed beneath it. The wire was then brought to a red heat by the current, and the fluids placed under it one after another. These experiments were made in a camera-obscura lined with black, and the result was entirely negative. They were then repeated by pouring portions of the fluids into test-tubes, holding them to the ignited wire, and looking in from the top. Under these circumstances also no fluorescence could be detected. Only the fluid No. 5 glittered with a reddish light, but this is its ordinary colour. The experiment showed that the light of the ignited wire contained many red rays. This fact agrees

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with the observations which I made by holding coloured papers under the wire.—Poggendorff's *Annalen*, vol. xcvii. p. 329.

EXAMINATION OF THE GREEN MATTER OF THE TRUE INFUSORIA. BY THE PRINCE OF SALM-HORSTMAR.

The author commences by stating that his former communication upon the green colouring matter of the Infusoria* was founded upon an error, as the objects investigated by him were not Infusoria but minute Algæ (*Coccodea viridis*). He has now investigated the green matter of *Euglena viridis*.

The animals, which were very lively, when collected on a filter, dried, and extracted with alcohol, furnished an emerald-green extract with a yellowish tinge, which gives a *blood-red dispersed light*. The extract evaporated to dryness at a gentle heat presented the following properties:—

It does not dissolve perceptibly in water, even when heated. Ammonia dissolves it when heated with a yellowish-green colour, and the solution is somewhat turbid. It is somewhat soluble in solution of caustic potash with the assistance of heat; the solution is yellowish-green.

Sulphuric æther dissolves it very readily with an emerald-green colour. This solution exhibits a very strong blood-red dispersion of both sun- and candle-light. It does not leave a coloured residue when left to spontaneous evaporation in an open test-glass, so that the coloured matter possesses the remarkable property of being volatilized with æther.

It dissolves readily in oil of turpentine with a green colour, and produces a blood-red dispersion of light.

The behaviour of this dry green matter obtained from the alcoholic extract, when heated in an open platinum cup, is very remarkable. Thus when it is gently heated, without bringing the platinum cup to redness, it evaporates without fusing, and diffuses an odour of fish. It leaves a blackish-brown residue, which gradually evaporates by heat (if the platinum cup be so heated as not to reach ignition), but does not take fire even when the platinum is heated to redness.

Behaviour of the green alcoholic solution towards Reagents.—The addition of an equal volume of water renders it slightly turbid at first. The turbidity is green; and on boiling, all becomes clear and green; even after the addition of five volumes of water it does not again become turbid after boiling, and forty volumes of water may then be added without perceptible turbidity.

Acetic acid produces a green turbidity. Acetate of lead also causes a green turbidity, which afterwards becomes a green precipitate; this is readily soluble in alcohol, giving a green colour without red dispersion. Acetate of copper causes no turbidity; when heated there is a slight turbidity.

Nitrate of silver gives no turbidity, but after standing for a few hours a greenish-black precipitate. The supernatant fluid retains

* See Phil. Mag. October 1855, p. 309.

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its green colour and red dispersion, but in about twelve hours it becomes pale and the precipitate increases.

Nitrate of lime produces no turbidity, but the colour becomes olive-green, and there is no longer any red dispersion. After standing twelve hours a precipitate is formed and the colour of the liquid disappears.

Muriate of alumina dissolved in alcohol gives a green turbidity, and the fluid loses its colour. After standing some hours a greenish, not flocculent, precipitate is formed.

Muriatic acid strikes an olive-green colour.

On litmus paper it has neither an acid nor an alkaline reaction.

The green colouring matter in *Euglena viridis* is therefore essentially different from that of the Algæ, as well as from the chlorophyll of the Phanerogamia, and of the green mosses.—Poggendorff's *Annalen*, vol. xcvii. p. 331.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1856.

Chiswick.—February 1. Light clouds: frosty. 2. Cloudy. 3. Frosty: fine throughout. 4. Overcast: slight rain. 5. Very fine: boisterous at night. 6. Densely clouded: boisterous. 7. Uniformly overcast: rain. 8. Densely overcast: fine: cloudy. 9. Exceedingly fine. 10. Cloudy. 11. Foggy: rain: overcast. 12. Rain: fine: rain at night. 13. Rain: showery throughout. 14. Rain: fine. 15. Cloudy: very fine: foggy at night. 16. Foggy: very fine: foggy. 17. Hazy: overcast: foggy at night. 18. Overcast: slight snow. 19. Hazy: cold and raw. 20. Cloudy and cold throughout. 21. Slight rain: small hail occasionally. 22. Overcast: slight rain. 23. Fine throughout: cloudy at night. 24. Fine: overcast: clear and frosty. 25. Overcast throughout. 26. Cloudy: slight rain. 27. Overcast. 28. Very slight drizzle: overcast: cloudy. 29. Foggy: cloudy: frosty.

Mean temperature of the month	41°·54
Mean temperature of Feb. 1855	28 ·01
Mean temperature of Feb. for the last thirty years	38 ·71
Average amount of rain in Feb.	1·543 inch.

Boston.—Feb. 1—3. Fine. 4, 5. Cloudy. 6. Rain A.M. and P.M. 7, 8. Cloudy: rain A.M. 9. Cloudy. 10. Cloudy: rain A.M. 11. Cloudy: rain P.M. 12. Rain A.M. 13. Cloudy: rain P.M. 14, 15. Fine. 16—22. Cloudy. 23, 24. Fine. 25—28. Cloudy. 29. Foggy.

Sandwick Manse, Orkney.—Feb. 1. Cloudy A.M.: showers, thaw P.M. 2. Cloudy A.M.: fine P.M. 3. Fine, bright A.M.: fine, clear P.M. 4. Fine, cloudy A.M.: fine, clear P.M. 5. Fine, drops A.M.: fine, cloudy P.M. 6. Bright A.M.: rain P.M. 7. Bright A.M.: showers P.M. 8. Cloudy A.M. and P.M. 9. Drops A.M.: clear P.M. 10. Bright A.M.: clear, showers P.M. 11. Bright A.M.: clear, fine P.M. 12. Rain A.M.: showers P.M. 13. Bright A.M.: cloudy P.M. 14. Snow-showers A.M.: showers P.M. 15. Rain A.M.: cloudy P.M. 16. Drizzle, showers A.M.: drizzle P.M. 17. Damp A.M. and P.M. 18. Cloudy A.M. and P.M. 19. Cloudy, frost A.M.: clear, fine P.M. 20. Bright A.M.: cloudy, fine P.M. 21. Bright A.M.: clear P.M. 22. Bright A.M.: showers, clear P.M. 23. Bright A.M.: cloudy P.M. 24. Cloudy A.M.: clear P.M. 25. Rain A.M.: showers, clear, aurora P.M. 26. Cloudy A.M.: drizzle P.M. 27. Showers A.M.: fine, cloudy P.M. 28. Drizzle A.M.: damp P.M. 29. Cloudy A.M. and P.M.

Mean temperature of Feb. for previous twenty-nine years	38°·01
Mean temperature of this month	40 ·84
Mean temperature of Feb. 1855	31 ·64
Average quantity of rain in Feb. for fifteen previous years	3·25 inches.

The storm which raged so violently in the South of Scotland on the 6th and 7th did not reach Orkney or the North of Scotland, but again we had this month as well as during the gale of last month a great fall of the barometer, which stood at 28·49 on the 6th at midnight.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1856. Feb.	Max.	Min.		9¼ a.m.	8¾ p.m.	Max.	Min.		9¼ a.m.	8¾ p.m.						
1.	30·136	30·051	29·81	29·93	29·84	38	29	25·5	37	38½	w.	w.	wnw.	.....	.....	·02
2.	30·001	29·968	29·66	29·85	29·91	38	28	29	35	35½	w.	nw.	se.	.....	.....	·36
3.	30·025	30·011	29·77	29·88	29·84	44	25	31	40	32	se.	se.	calm	.....	.....	.....
4.	29·995	29·952	29·66	29·75	29·72	42	32	34	36½	36	sw.	s.	e.	·01	.....	.....
5.	30·137	30·093	29·74	29·68	29·42	52	41	36	42½	40½	sw.	ssw.	sse.	·02	.....	.....
● 6.	29·927	29·612	29·45	29·21	28·75	50	47	47	42½	36½	sw.	sw.	e.	·02	.....	·05
7.	29·912	29·662	29·24	29·19	29·18	56	47	47·5	42	40	sw.	sw.	s.	·06	·10	·23
8.	30·038	29·986	29·50	29·34	29·45	50	45	47·5	46½	47	sw.	sw.	ssw.	.....	·02	·16
9.	29·972	29·909	29·50	29·42	29·44	60	45	44	45½	44	sw.	sw.	w.	.....	·03	·01
10.	29·962	29·923	29·60	29·62	29·75	54	41	46·5	44½	43	s.	sse.	wsw.	.....	.....	.....
11.	29·861	29·810	29·50	29·78	29·81	53	42	46	44	40½	sw.	sw.	e.	·20	·15	·03
12.	29·737	29·375	29·31	29·73	29·60	50	43	47	40	38½	sw.	s.	e.	·05	·42	·09
) 13.	29·773	29·685	29·40	29·79	29·79	53	43	45	39	37	s.	s.	e.	·20	·02	·01
14.	29·739	29·709	29·30	29·58	29·51	54	40	47	35	36	sw.	ssw.	e.	·02	·26	·05
15.	29·773	29·726	29·34	29·60	29·77	55	32	41·5	37½	38	sw.	s.	e.	.....	.....	·71
16.	29·841	29·765	29·47	29·80	29·83	54	32	42·5	39	40	e.	s.	se.	.....	.....	·06
17.	29·843	29·831	29·58	29·85	29·94	43	34	41	43	40	e.	ese.	sse.	.....	.....	·28
18.	29·762	29·707	29·55	30·01	30·11	36	33	34	37	34½	e.	e.	ese.	·01	.....	.....
19.	29·867	29·753	29·54	30·16	30·17	42	32	35	35	31½	ne.	ne.	se.	.....	.....	.....
20.	29·908	29·841	29·62	30·13	30·28	38	32	34	37	38	ne.	n.	calm	.....	.....	.....
○ 21.	30·054	29·916	29·77	30·22	30·02	37	28	33	40½	41	ne.	ne.	wnw.	·01	.....	.....
22.	30·047	29·976	29·70	29·95	29·90	43	27	34	43½	43	nw.	nw.	wnw.	·01	.....	.....
23.	30·229	30·017	29·70	30·01	30·08	50	27	37·5	45	45	n.	w.	w.	.....	.....	·03
24.	30·458	30·362	30·00	30·12	30·16	50	25	36	45	42	sw.	w.	wsw.	.....	.....	·01
25.	30·467	30·406	30·07	29·88	29·81	47	41	41·5	44½	44½	sw.	w.	w.	.....	.....	·20
26.	30·369	30·314	29·87	29·94	30·06	57	45	47	46	45½	w.	sw.	w.	·01	.....	·05
27.	30·473	30·394	29·99	30·18	30·18	51	42	47·5	46½	46	nw.	wnw.	sw.	.....	.....	·04
28.	30·183	30·463	30·10	30·22	30·36	49	36	47	47	45½	n.	sw.	wsw.	·01	.....	·03
(29.	30·590	30·473	30·13	30·40	30·45	52	28	40	48	45½	ne.	n.	sw.	.....	.....	.....
Mean.	30·037	29·951	29·65	29·835	29·832	48·20	34·89	40·1	41·51	40·17				0·63	1·00	2·42

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MAY 1856.

XLI. *On a peculiar case of Colour Blindness.* By JOHN TYNDALL, *F.R.S. &c.**

A CASE of colour blindness has been recently brought under my notice by Mr. White Cooper, of so singular a character that I think even the brief description of it which the pressure of other duties permits me to give will not be without interest to the readers of the Philosophical Magazine.

Out of eleven hundred and fifty-four cases examined by Dr. George Wilson of the University of Edinburgh, and recorded by him in his truly interesting and valuable work on Colour Blindness†, only one instance was found in which the sufferer was aware of the loss he had sustained. This was the case of a medical practitioner in Yorkshire, who in November 1849 was thrown from his horse. "After rallying from the collapse which immediately succeeded the accident, he suffered from severe pain in the head, delirium, mental excitation approaching almost to mania, loss of memory, and other symptoms of cerebral disturbance. . . . . On recovering sufficiently to notice distinctly objects around him, he found his perception of colours, which was formerly normal and acute, had become both weakened and perverted, and has since continued so. . . . Flowers have lost more than half their beauty for him, and he still recalls the shock which he experienced on first entering his garden after his re-

* Communicated by the Author.

† At page 137 I find some observations of mine referred to. I would here remark, that whatever observations or experiments I have hitherto made on this subject were merely repetitions or modifications of those of Dove or Helmholtz, whose excellent memoirs I had the pleasure of introducing into this country.

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covery, at finding that a favourite damask rose had become in all its parts, petals, leaves, and stem, of one uniform dull colour; and that variegated flowers, such as carnations, had lost their characteristic tints."

The case of Captain C., which I have to describe, is one of these rare instances. The sufferer is a seaman, and ten or twelve years ago was accustomed, when time lay heavy on his hands, to occupy it by working at embroidery. Being engaged one afternoon upon a piece of work of this description, and anxious to finish a flower (a red one, he believes), he prolonged his labours until twilight fell, and he found it difficult to select the suitable colours. To obtain more light he went into the companion, or entrance to the cabin, and there continued his needlework. While thus taxing his eyes, his power of distinguishing the colours suddenly vanished. He went upon deck, hoping that an increase of light would restore his vision. In vain. From that time to the present he has remained colour blind.

My first examination of Captain C. took place in the house of Mr. Cooper. Being furnished with specimens of Berlin wool, such as that with which the patient had been accustomed to work, I placed before him a skein which he at once, and correctly, pronounced to be blue. For this colour he has a keen appreciation, and I have never known him make a mistake regarding it. Two bundles of worsted, one a light green and the other a vivid scarlet, were next placed before him: he pronounced them to be both of the same colour; a difference of shade was perceptible, but both to him were drab. A green glass and a red glass were placed side by side between him and the window: he could discern no difference between the colours. A very dark green he pronounced to be black; the purple covering of the chairs were also black; a deep red rose on the wall of the room was a mere blotch of black; fruit, partly of a bright red and partly of a deep-green, were pronounced to be of the same uniform colour. A cedar pencil and a stick of sealing-wax placed side by side were nearly alike; the former was rather brown, the latter a drab. Time, I found, made a difference: slate colour and red were placed side by side; on first looking at them Captain C. thought them different shades of the same colour, but after looking at them for half a minute even this difference of shade disappeared. By the production of subjective effects, such as looking long at an object through a coloured glass, and then removing the latter, his judgement of colours could also be made to vary in a slight degree.

My second examination of Captain C. took place in the theatre of the Royal Institution; and on the day he called upon me I happened to be using the electric light, rendered continuous by

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Duboscq's lamp. A portion of the light was permitted to pass through a bright green glass and was received upon a screen; no change of colour was perceived: the space on which the green light fell was merely a little less intensely illuminated than the remaining portion of the screen. Lycopodium was shaken upon glass: the electric light looked at through such a glass gives, as is known, a series of brilliantly coloured rings: to Captain C., however, no colour was manifest, merely light and obscure rings following each other in succession. A spectrum was cast upon the screen in which all the prismatic colours shone vividly; to Captain C. only two colours were manifest, namely blue and whitish-yellow. The outline of the spectrum was the same to him as to me; all that gave me light gave him light also; but in his case the red, orange, and green were so modified as to produce the uniform impression of whitish-yellow. In some cases of colour blindness, where the sufferer confounds red with green, it is difficult to say whether he takes the red for a green or the green for a red. In the present case neither of these expresses the fact; neither red nor green is seen, but both of them are reduced to a colour different from either.

Captain C. assured me, that, previous to the circumstance related at the commencement, he was a good judge of colours, so that in pronouncing upon any colour he has an aid from memory not usually possessed by the colour blind. Indeed I had myself an opportunity of reviving his impression of red. A glass of this colour was placed before his eyes while he stood close to the electric lamp: on establishing the light, he at once exclaimed, "that is red!" He appeared greatly delighted to renew his acquaintance with this colour, and declared that he had not seen it for several years. The glass was then held near the light while he went to a distance, but in this case no colour was manifest; neither was any colour seen when a gas-lamp was regarded through the same glass. The intense action due to proximity to the electric light appeared necessary to produce the effect. "You gave the eye a dram," observed a gentleman to whom I described the case: the figure appears to be a correct one. Captain C.'s interest in this experiment was increased by the fact, that the Portland light, which he has occasion to observe, has been recently changed from green to red, but he has not been able to recognize this change. The fare in the fore-cabin of a vessel of his own which he now commands happens to be sixpence, and he is often reminded by the passengers that he has not returned their change. The reason is, that he confounds a sixpence with a half-sovereign, both being to him of the same colour. A short time ago he gave a sovereign to a waterman, believing it to be a shilling.

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It was my intention to make a guess at the cause of colour blindness in the case above described; but guesses, without the means of verifying them, are so unsatisfactory, and so apt to produce fruitless discussion, that for the present at least I will confine myself to the statement of the facts.

Two other cases of a different nature were also brought under my notice by Mr. Cooper, and may, on account of their rarity, be worthy of a brief reference.

The first is that of a little girl, about seven years old, the development of whose eyes had been arrested before birth. The child's sight, however, though imperfect, was sufficient to enable her to distinguish colours with accuracy. When the spectrum was displayed before her, she ran her fingers promptly over the colours and named them correctly. She could also read large print. The phænomena of irradiation presented themselves to her as they did to me; an incandescent platinum wire became thicker as she receded from it. As far as I could judge, the retina was perfectly healthy. I placed her within a foot of the coal-points of the electric lamp, and establishing the current, allowed the full splendour of the light to fall upon her eyes: she never even winked, but looked steadily into the light, and stated that she did not feel the slightest inconvenience. This perhaps was due to the partial opacity of the humours of the eye. The position of the iris in her case was marked by a few gray spots, and the pupil had no definite boundary. The eyes were, as might be expected, out of all proportion with the growth of the child: the arrestation of development extended to the teeth also, which caused the child to appear much older than she really was. She was very intelligent; and her mother, who accompanied her, was a healthy intelligent woman, with fine brown eyes. She stated to me, that neither in her own nor her husband's family did a case of the kind ever occur; and yet she had four children, and the whole of them, without exception, were afflicted in a similar manner.

The second case is that of a distinguished artist, also sent to me by Mr. Cooper. Several months ago he noticed, on looking at any distant point of light, a whitish luminosity spreading round the point, and first observed this appearance on the occasion of rubbing his right eye somewhat severely. As time advanced, the luminosity merged into a series of coloured rings which encircled the luminous point; and as these were becoming brighter and larger, his fears of the ultimate blindness of the eye became excited. He had consulted several eminent oculists, and had, I believe, been subjected to severe treatment, on the supposition that the retina was the seat of the malady. The coloured curves were not perfect circles. I placed Mr. S. upon his

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knees on the floor, and caused him to look upward at the electric lamp: in this position the upper portion of the pupil was shaded by the eyelid, and the coloured rings totally disappeared. I then caused him to stand upon a table and to look down upon the lamp: in this position the under portion of the pupil was shaded by the lid, and the colours were displayed in all their brilliancy. Mr. S.'s left eye was totally free from all defects of this kind. I shook a little lycopodium on glass, and presented it before his left eye. The system of rings this revealed to his good eye was precisely similar to those presented to the other. The lycopodium rings were smaller, but in other respects the same as those of the right eye, with the exception of the divergence of the latter from the circular form mentioned above. I ventured to express my doubts to Mr. S. as to the retina being the seat of the disease, and to comfort him with the hope that the augmentation of the rings in brilliancy and magnitude pointed rather to the diminution than to the increase of his malady. I will leave it to physiologists to say what possible particles within the humours of the eye could act the part of the spores of lycopodium without the eye; but I entertain very little doubt that it is from the presence of such particles, a thin film, or some equivalent optical cause, and not from any affection of the retina, that the effects observed by Mr. S. arise. If this be the case, it simply shows how necessary a knowledge of physics is to medical men. I now regret that want of time prevented me from entering further upon the examination of the case last referred to.

Royal Institution,
April 1856.

With reference to the case of Captain C., Mr. Cooper makes the following remarks:—"In this case the symptoms are clearly referable to the intense strain to which the eyes were subjected for a long period, and under unfavourable circumstances—a strain beyond endurance, and which seems to have deprived the retina of the power of appreciating impressions. Such a condition is little amenable to treatment. After the Great Exhibition of 1851, instances came under my notice in which the sensibility of the retina was temporarily blunted by the excitement to which it was exposed in that brilliant scene. Here the sensibility to impressions of colours was only suspended, and gradually returned; but it is to be feared, that, in the case narrated by Professor Tyndall, it may be regarded as extinguished: the vibrations of the coloured rays produce no responsive action in the nervous fibrillæ."—W. C.

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XLII. *Researches on the Methods of preserving the Sensitiveness of Collodion Plates.* *By* JOHN SPILLER *and* WILLIAM CROOKES.*

IT is now nearly two years since we had the honour of presenting to the photographic world our first experiments made with the view of preserving the sensitiveness of collodion plates. In the Philosophical Magazine for May 1854 (an abstract appearing in the Photographic Journal of that month) we communicated the possibility of securing this end by taking advantage of the deliquescent nature of certain neutral salts, which, by retaining water in the film, enabled us to prolong or defer the exposure of the sensitive plate for a length of time which was not practicable by the ordinary collodion process. For this purpose we proposed the use of the nitrates of zinc, manganese, lime or magnesia, and, as the type of a class of substances equally suitable in the organic kingdom, glycerine; sugar also had been tried, but with no good result. In consequence of public attention being again drawn to glycerine, by the lecture recently delivered before the Society of Arts, we think it but right to assert our claim of priority in suggesting the application of this body to the purpose under consideration. We quote from the article of May 1854. "Glycerine at first seemed to promise very good results, but the principal difficulty was the necessary impurity of the commercial product, in consequence of its being obtained from the exhausted leys of the soap boilers." Now, however, that an improved process of manufacture has been introduced at the works of Price's Patent Candle Company, where it is obtained as a bye product in the decomposition of fatty matters by high-pressure steam, it became a point of interest to determine whether the purer article might not well serve the object in view. With this intention we procured a sample of Price's glycerine as soon as it became an article of commerce, and although the result of our experiments coincides to a certain extent with those of Mr. Pollock and others, we nevertheless think it worth while to specify the particular points of difference in manipulation, some of which will, we believe, materially facilitate the preparation of the plates in this way.

Our first care was to ascertain the action of glycerine upon an aqueous solution of nitrate of silver. For this purpose, a mixture was made and divided into two portions, one of which was exposed to a full southern aspect, and the other carefully protected from every gleam of light; after a few days a thin but distinct coating of metallic silver was found lining the interior of the

* Communicated by the Authors.

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glass vessel in the light, while very slight, if any, evidence of reducing action was appreciable in that kept in the dark, even after the expiration of a month. Finding the action of light to exercise this influence, we determined to keep separately, as far as possible, these two necessary ingredients in the process. With regard to the degree of concentration of the glycerine, the sample made use of has a specific gravity of 1·23; this we have employed in its original state, and mixed with various proportions of water; we perceive no great difference in the results, but are inclined to prefer its employment with but little dilution with water.

The process we have been led to adopt is the following:—The glass plate, cleaned with especial care (by treatment, first, with a hot solution of common washing soda, and subsequently with strong nitric acid), is coated with iodide of ammonium collodion in the usual way, and made sensitive by immersion in the ordinary silver bath (30 grains of the nitrate to 1 ounce of water; perfectly saturated with iodide of silver*, so that the plate may be left its full time without fear of dissolving the sensitive film); after remaining here three or four minutes, the excited plate is transferred to, and immersed for an equal time in, a washing bath of pure distilled water; or instead of this bath we have sometimes used a stream of water from the "syringe bottle," the object being to remove the great excess of free nitrate of silver from the sensitive film.

So prepared, the plate is ready to receive the glycerine treatment. For this purpose we require, besides Price's glycerine, sp. gr. 1·23 or thereabouts, a dilute solution of nitrate of silver (one grain of nitrate to the ounce of water). When about to be used, an *intimate* mixture is made in the proportion of three parts by volume of glycerine to one of the silver solution, and poured on to the surface of the washed collodion plate, its action being assisted by transferring, some two or three times, to and from the measure glass; after five minutes' contact the plate has to be well drained, and placed in a nearly vertical position on blotting-paper, to absorb the large excess of glycerine from its surface. It will then be in a fit state for receiving the impression in the camera, a process which may either be performed immediately or deferred for a period of at least twenty-four days, the longest trial to which we have as yet submitted the plates.

* This is readily effected by dissolving the total weight of nitrate of silver in one-fourth of the bulk of water to be ultimately employed; a grain or so of iodide of potassium dissolved in a little water is now added, to precipitate an equivalent amount of iodide of silver, with which the solution will be saturated on stirring; the remaining bulk of the water is then added. After allowing time for subsidence, the solution may be filtered without difficulty. The addition of a few drops of glacial acetic acid to a large bath is an improvement.

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In regard to sensitiveness, they will, if used immediately, be found very little inferior to plates prepared in the ordinary way; we have, however, detected evidence of slight deterioration in proportion to the length of time the exposure has been deferred. In cases where it is necessary to keep the plates ready excited through a protracted interval, we have devised a convenient plate-box to store them in, which may easily be made by replacing the wooden grooves in an ordinary plate-box by two corrugated sheets of gutta percha, and laying a square of thin caoutchouc at the bottom for the glasses to rest upon. Such a box will always require an outer covering to protect its contents from every gleam of light, the necessity for which precaution, as also that of excluding injurious gases, such as ammonia and sulphuretted hydrogen, will be sufficiently obvious without further comment.

Before proceeding to develope the latent image on the glycerine plate, it is only necessary to immerse it for two or three minutes in the 30-grain nitrate of silver bath, when the solution of pyrogallic acid or protosalt of iron may be applied as usual; the remaining part of the process, fixing, &c. being conducted in the ordinary manner.

The negative pictures resulting from this mode of treatment have not, in our hands, been found wanting either in intensity or in gradation of tone; they are in fact fully equal to the results of the collodion process as usually practised.

If considered desirable, a bath of the mixture of glycerine and nitrate of silver may be employed, instead of the mode of application recommended above; in that case it will be necessary to protect the fluid from the light, so as to avoid the deposition of metallic silver; and on that account, to make use of a covered *gutta-percha* in preference to a *glass* bath for containing the solution. The remarkable purity of Price's glycerine, its absolute freedom from chlorides (and sulphates), renders the plan of mixing only as required for use far more practicable than would otherwise have been the case, had filtration been necessary. Any excess of the preservative fluid, remaining after the preparation of a certain number of the plates, should be kept on stock (in a dark place), and may be again employed for the same purpose, after filtering and adding a little pure glycerine to counterbalance the accession of a small proportion of nitrate of silver from each successive plate.

In addition to the glycerine process, we have, at intervals, given some attention to the other means of preserving collodion plates, and have succeeded in attaining that object by several other methods, as also in improving the processes already detailed in our former communications.

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Bearing in mind the qualities requisite to fulfil in the best manner the functions of a preservative agent, it occurred to us that it might be possible to find a body having in itself the power of rendering the collodion film sensitive to light, or at least of sustaining it in that condition, and at the same time possessing deliquescent properties; substances having these two characters combined are presented in the fluoride and silico-fluoride of silver. To put this supposition to the test of experiment, we prepared these compounds (by dissolving freshly precipitated carbonate of silver in hydrofluoric and hydrofluosilicic acids respectively), and used their solutions, in place of the ordinary nitrate of silver bath, for exciting the iodized collodion film. Plates so treated readily became coated with a layer of iodide of silver, which seemed to be equally sensitive to light, whether produced by this or the method commonly employed; they had also the power of retaining a moist surface; but, unfortunately for the present object, it was found that a strong solution of fluoride of silver, like that of the nitrate, has the property of dissolving off the precipitated iodide of silver, destroying it by forming the small holes so well known in the ordinary collodion process. Meeting with this result, we determined to try their application in a more dilute form, after exciting the plate in a preliminary nitrate of silver bath; but by this mode of treatment also we were unsuccessful, being unable to preserve the sensitiveness by a quantity which was insufficient to exercise a destructive influence on the film. This difficulty, added to that experienced in the preparation of the fluoride in a neutral condition, any excess of hydrofluoric acid being objectionable on account of its property of etching the glass, and on the other hand, the slightest alkaline reaction rendering it extremely difficult to obtain clear pictures on development, deterred us from pursuing the subject further in this direction.

Compelled, therefore, to return to the principle originally adopted, viz. the use of nitrate of silver in conjunction with a deliquescent salt, sufficient in amount to prevent its crystallizing or even concentrating beyond a certain limit upon the plate, we sought only to improve the processes already before the public.

A longer experience with the nitrate of magnesia has demonstrated the difficulty of preparing this substance on a large scale, free from an impurity very inimical to its successful application —the nitrite of magnesia; the presence of this latter, by giving rise to the formation in the film of the nitrite of silver, a body prone to spontaneous decomposition even at ordinary temperatures, must necessarily introduce a condition unfavourable to the ultimate result. To effect the conversion of the nitrite* into

* The presence of nitrous acid is easily recognized by Dr. Price's test. It is applied by adding a mixture of dilute hydrochloric acid and iodide of

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nitrate, and at the same time to neutralize the invariable alkalinity of the commercial substance, we prefer to employ nitric acid highly diluted, and added gradually to the magnesian salt, previously dissolved in water, until a very faintly acid reaction is communicated to blue litmus paper; any decided excess of acid must be avoided, its presence being certainly antagonistic to a high state of sensibility.

The proportions we have generally adopted are,—

Nitrate of magnesia . . .	4 ounces.
Nitric acid	quant. suf.
Nitrate of silver	12 grains.
Water	12 ounces.

The silver salt must be added after the neutralization has been performed, any precipitated chloride resulting from impurity being removed by filtration. Before use it should be ascertained that the solution really contains silver, by transferring a few drops of the clear fluid to a watch-glass, and mixing with common salt, when a milky turbidity, however slight, will indicate the presence of a sufficient amount of silver to sustain the sensitive condition of the plate.

The solution of nitrate of magnesia may, if preferred, be prepared by double decomposition between sulphate of magnesia and nitrate of baryta, mixing them in the proportion of their chemical equivalents, and filtering off the insoluble sulphate of baryta. The only advantage in practising this method is the certainty of obtaining a neutral solution when the pure crystallized salts have been employed; it will, however, be found impossible to exclude a slight excess of one or other of these salts; a small quantity of sulphate of magnesia was left in the solution used in our experiments, but it did not appear to exert any injurious influence. A small proportion of nitrate of silver must as usual be added before use.

The double nitrate of magnesia and ammonia we have also employed with very good results. It was prepared by measuring out two equal volumes of diluted nitric acid, saturating the one with carbonate of magnesia, and the other with carbonate of ammonia, and then mixing; the solution required the addition of a few drops of very weak nitric acid to render it neutral, and a small quantity of nitrate of silver.

Nitrate of manganese, prepared either by dissolving the precipitated carbonate in dilute nitric acid, or by double decomposition between equivalent quantities of crystallized sulphate of manganese ($MnO, SO^3 + 4HO$) and nitrate of baryta, gives, upon

potassium, with a little starch paste, to the nitrate of magnesia dissolved in water; in the event of its containing *nitrite*, a blue colour or precipitate will appear, according to the amount of this impurity that may be present.

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addition of a small proportion of nitrate of silver, a solution well fitted for use as a preservative agent. The colour of the liquid is a pale rose-red. The nitrate of copper has also been tried for our purpose, but did not give promising results, the sensitiveness of the collodion film being greatly impaired by the highly acid nature of this salt.

Finally, we have employed with excellent results the nitrate of nickel, which, however, requires some care in its preparation. The method we have found most successful consists in dissolving the metal in the smallest possible quantity of nitric acid, and adding to the solution highly dilute aqueous ammonia, sufficient in amount to precipitate a small portion of the oxide of nickel; this being filtered off, the liquid will have an alkaline reaction: nitric acid is now added until nearly neutralized, and the last traces of alkalinity removed by acetic acid, a slight excess of which is an advantage. Nitrate of silver should now be introduced in the proportion of 2 per cent. of the nickel originally employed. The above mode of proceeding will obviously give rise to the production of a certain quantity of nitrate of ammonia; this, however, combines to form a double nitrate of nickel and ammonia, a salt possessing deliquescent properties, and apparently equally suitable for our purpose. The colour of this agent, bright green, suggests the possibility of employing it with advantage in cases where green foliage has to be represented in juxtaposition with objects reflecting more active photographic rays.

Of all the substances known to be applicable to the preservation of collodion plates, we believe that the use of glycerine will give less trouble to those unaccustomed to chemical manipulation, and will be generally preferred from the greater certainty of its results. We have nevertheless thought it worth while to record our experience in respect to the other agents severally enumerated, even where, as in the case of fluoride of silver, they have not led to successful results, believing that a statement of the conditions under which we have endeavoured to employ them may save loss of time to future experimentalists in the same direction.

London, April 14, 1856.

XLIII. *On a General Law of Electrical Discharge.* By Sir W. Snow Harris, *F.R.S.**

[With a Plate.]

1. In a memoir on Electrical Accumulation, presented to the members of the Plymouth Philosophical Institution so long since as the year 1826, and printed in a volume of their

* Communicated by the Author.

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Transactions for 1830, I announced a law of electricity of great generality, viz. that the heating effect of the ordinary electrical discharge transmitted through a metallic wire placed in the circuit is as the square of the accumulation, and is entirely dependent on the quantity of electricity discharged, without any regard to the intensity indications of the ordinary electrometers. Consequently through whatever interval of air the discharge can pass, as measured by a Lane's electrometer, the heating effect of the momentary current in the wire will, with a similar circuit, be always the same for the same quantity of electricity,—a deduction which has since been fully verified by further discoveries in electricity*. M. De la Rive, for example, found the heating effect of the voltaic current, and which he estimated by the beautiful helical thermometer of Brequet, wholly dependent on the quantity of electricity†. Faraday also shows, Experimental Researches (366.), that "if the same quantity of electricity pass through the galvanometer, the deflection of the needle is always the same whatever may be the electrical intensity;" and again (704.), in the case of electro-chemical decomposition, "the force of a given quantity of electricity is always the same, notwithstanding the greatest variations of intensity." Thus the law which I had previously announced has been so far satisfactorily confirmed by the subsequent investigations of two philosophers, whose admirable researches have enriched this branch of physics.

2. Although my original announcement is thus sanctioned by experimental evidence of the highest authority, yet M. De la Rive, in his valuable work, *Traité de l'Electricité*, recently published, has quoted largely from certain memoirs of M. le Professeur Riess of Berlin, who thinks he has shown the fallacy of my announcement, "la fausseté de cet énoncé." Repeated experiments, he says, "have informed me that the elevation of temperature in a metallic wire by the electrical discharge is proportional to the quantity of electricity accumulated multiplied by its density, or what comes to the same thing, proportional to the square of the quantity divided by the extent of the battery ‡;" so that his formula would be $T = \frac{Q^2}{s}$, in which T is the elevation of temperature, Q the quantity, and s the extent of the battery. At p. 116, M. Riess attributes my failure in arriving at the same conclusion as himself, to a want of accuracy in my experiments,

* See also Phil. Trans. for 1834, p. 225.

† *Ann. de Chim. et de Phys.* vol. liii. p. 177 and 183.

‡ *Ann. de Chim.* vol. lxix. p. 113; and De la Rive, *Traité de l'Electricité*, vol. ii. pp. 154 and 162.

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and the imperfect construction of my instrument, viz. the "thermo-electrometer*."

3. As this is a question of much physical interest, more especially in the present progressive state of electricity as a science, and as any observation of so skilful and profound a philosopher as M. Riess merits great consideration, more especially when countenanced by so distinguished a writer as M. De la Rive, I am desirous to submit, for the consideration of those engaged in electrical inquiries, some further remarks and experiments relative to the exceptions thus taken to the accuracy of my early announcements. I am led to take this step, being under the impression that I may thereby benefit science, and throw some further light on this interesting subject. I think it will be found that M. Riess, on referring to my inquiries, has not clearly apprehended the phænomena in question, so prominently set forth by M. De la Rive. I may perhaps further claim, and not unreasonably, the privilege of seeking to obtain some little justice for myself, in reference to the remarks of M. Riess, viz. that my experiments have proved "unfruitful," and that he has "shown the falsity of my announcement," observations which seem to imply a belief in the infallibility of his own deductions, and which he thus erects into a sort of standard of value with which to compare mine. I think, however, it will be found that the results arrived at by M. Riess, and quoted by M. De la Rive, are really no others than those which I had myself previously obtained and published in my memoir above mentioned (1); and I trust to be enabled to satisfactorily explain in what consists the difference in our interpretation of these results.

4. First, however, I must beg to be allowed to submit a few brief explanatory observations on the nature and construction of my instrument—the thermo-electrometer, which M. Riess imagines to have been so imperfect in my hands as to induce him to place it under what he supposes to be a new form. The instrument has been much used, not only in this country but also on the Continent, and has without doubt rendered good service to the progress of electrical research; it is, in fact, upon this instrument that much of the real value of M. Riess's inquiries depend.

The thermo-electrometer, Plate III. figs. 1, 2, 3, was first invented by me so long since as the year 1820, now more than thirty-five years since, although M. De la Rive conceives he was the first person who employed a contrivance of this kind†. My original instrument will be found in a quarto work, which I published in 1823, relative to the effectual protection of the British Navy from

* *Ann. de Chim.* vol. lxix. p. 113; and *Traité de l'Electricité*, De la Rive, vol. ii. p. 154.

† *Traité de l'Electricité*, vol. i. p. 31.

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lightning*, where it is figured and described. Fig. 1 represents this original construction, in which *apvn* is the glass thermometer ball, about 4 inches in diameter, and *pn* a fine metallic wire passed air-tight across its centre. This wire is hermetically fixed through the ball by means of short metallic studs, attached to plates of metal cemented in and about two holes drilled on opposite sides through the glass; the studs are perforated for the passage of the wire, and are covered by flattened balls of metal screwed air-tight over them. *ab* is the thermometer tube, having a divided scale attached to it; its lower extremity, *b*, is sustained in a reservoir of glass, *w*, containing a coloured liquid. The instrument is prepared for experiment by first drawing out a small portion of air from the ball, *apvn*, by means of heat, and then immersing the lower extremity, *b*, of the tube in the coloured fluid; as the ball cools, the fluid ascends along the scale; its precise position is regulated by a small valve, *v*, cemented in a hole drilled through the upper part of the tube, in the way already described.

When an electrical discharge of a given force is passed through the wire, the fluid descends along the scale and marks the comparative degree of heat excited in the wire. For the better adjustment of the fluid to the zero-point of the scale, the latter is so contrived as to be moveable on the thermometer tube. The method of fixing the wire is shown in fig. 2, in which *pm* and *qn* are the metallic plates and balls already described, fig. 1; the plates being formed to the curvature of the glass, and firmly cemented to its surface by good sealing-wax. The metallic wire, *pn*, fig. 1, being first passed through the holes in the brass studs and put gently on the stretch, is secured in place by small plugs of wood, which, pressing the wire against the metal, not only secures it in the hole, but ensures a good metallic contact; the whole is rendered air-tight by the balls *pn*, which are flattened and screwed upon studs against the plates, from which they project, a fine washer of leather being interposed: the small valve *v* is fixed in the same way. The electrical discharge is caused to traverse the wire *pn* by means of metallic connexions inserted into holes drilled in the balls *pn*.

5. Although I found this form of the instrument very sensible and efficient as to its operation, yet it was not sufficiently convenient in practice. I was hence led to bend the thermometer tube so as to place it in a horizontal position, as shown in figs. 3 and 4, the extremity, *b*, of the tube being either bent downward into the vase containing the coloured liquid, as in fig. 3, or otherwise bent upward and expanded into a small ball open to the

* Letter to Vice-Admiral Sir T. B. Martin, K.C.B., Comptroller of Her Majesty's Navy, &c. Nicol and Co., London, 1823.

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external air, as in fig. 4; and as the whole was sustained upon a horizontal plane of wood, having free motion upon a similar plane beneath, either extremity of the instrument could be elevated within certain limits, so as to give the tube of the thermometer a greater or less degree of inclination. This is, in fact, the form of my instrument resorted to by M. Riess, and figured by M. De la Rive in his recent work, *Traité de l'Electricité*, vol. ii. p. 156; as also by M. Pouillet, *Elémens de Physique*, who refers the instrument to M. Knockenhauer.

In the course of my experiments with this form of the instrument, I sometimes employed long and delicate tubes containing a very small line of coloured fluid, moveable within them as an index merely. I also occasionally used a small column of mercury in the same way. After all my efforts, however, to perfect the instrument, I found no form so really efficient and simple as that of the instrument shown in fig. 5. In this figure, *pvnd* is the thermometer ball as before, capped and screwed at *d* upon a reservoir of coloured fluid, *dc*, and from which the thermometer tube is extended. The tube *cab* is twice bent, so as to bring it into a vertical position, *ab*, parallel with the reservoir, *cd*; the whole is mounted upon a small elliptical base of wood, sustained on three or four screw feet, as shown in the figure; the vertical portion of the tube, *ab*, being secured to, and sustained by, a rigid divided scale fixed to a support of wood springing out of the elliptical base.

The point *o* of the level of the liquid in the tube is marked zero on the scale. When an electrical discharge of a given force traverses the wire *p n*, the fluid is observed to ascend along the scale, indicating the comparative degree of heat excited in the wire. It is to be observed that the diameter of the reservoir, *cd*, which is a sort of hollow flattened ball, is sufficiently great to render the difference of the level of the fluid in the reservoir, caused by the abstraction of the quantity which ascends in the tube along the scale, infinitely small.

This form of the instrument is figured in the Transactions of the Royal Society for 1827, in the Memoirs of the Plymouth Institution for 1830, and also in the Transactions of the Royal Society of Edinburgh for 1834*, where it will be found applied to the purposes of voltaic electricity. When we employ a long fine wire of platinum in the ball turned into a helix, the instrument becomes marvellously sensitive. In order to facilitate experiments with different metals, I have sometimes employed a ball pierced in many points of its circumference for the reception of several wires, as shown in fig. 6. I also employed a cylindrical bulb, shown in fig. 7, in which the wires could be either fixed

* Vol. xii.

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one over the other, or otherwise wires of various lengths introduced, by fixing them either straight or curved between the two upper holes, or by placing them diagonally between the upper and under holes,—a practice pursued by some of the continental philosophers, and indicated and figured by M. Pouillet in his work, *Elémens de Physique*.

6. Having thus, for the sake of clearness, described and explained my instrument, such as it was when first invented and afterwards perfected, it may not be unimportant to quote some striking instances of its practical application. It has been well observed by the justly celebrated English chemist, Sir H. Davy, that "nothing is more important to the progress of science than the invention and application of a new instrument," that "the intellectual faculty is not more the source of success in physical discovery than the nature of the means which we are led to employ*." In the first place, however, I may observe that there is really no well-established law of electricity with which the indications of my instrument are not in perfect accordance, the results arrived at by M. Riess not excepted, as I shall presently show. This understood, it is to be further observed, that it was through the agency of this instrument, fig. 5, that Faraday first observed the heating powers of the magneto-electric current during the meeting of the British Association at Oxford in 1832†. The heating effect of the shock of the Gymnotus was first observed with this instrument at the Adelaide Gallery in London, in 1839, by Mr. Gassiot and myself. In the course of our experiments we employed a fine silver wire turned into a helix, as first suggested by Mr. Gassiot. Dr. Davy, F.R.S., describes in his most interesting work, entitled 'Physical and Anatomical Researches,' the great success of my instrument in rendering sensible the heating effects of the shock of the torpedo: he says, "the sensibility of this instrument is so great, that the spirit in the stem was not only moved by a single spark from the electrical machine, but even very distinctly by the electricity of a single voltaic combination composed of copper and zinc wire; the former $\frac{1}{23}$th of an inch in diameter, the latter $\frac{1}{50}$th, excited by dilute sulphuric acid." This instrument, he further observes, "was strongly affected by active fish, and even distinctly by weak ones; indeed, occasionally, when it formed part of a circle in connexion with the galvanometer, I have seen it affected alone, the galvanometer affording no indication." Dr. Davy, in his experiments, employed an exceedingly fine wire of platinum, drawn down by Wollaston's method, described in the Philosophical Transactions for 1813; he employed also a stopcock for regu-

* "Elements of Chemistry."

† Faraday's Experimental Researches, 3rd series, p. 98.

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lating the altitude of the spirit in the stem, and used as small a quantity of spirit as possible*. Here is, as must be allowed, very strong evidence as to the sensibility of my instrument at least; and upon the whole I am led to doubt the great superiority and advantage claimed by M. Riess for the particular construction of my instrument which he employed, and which is figured by M. De la Rive in his recent work, vol. ii. p. 156.

7. Having thus explained and illustrated the application and use of the thermo-electrometer, and which was invented full thirty-five years since, I shall endeavour, for the objects of science, to show its exact accordance with the formula of M. Riess when correctly interpreted, as well as its great applicability to the purposes of electrical research. In order, however, to avoid any misapprehension, we will revert first, in express terms, to the question we are about to consider. My announcement was, that the heat excited in a metallic wire by the electrical discharge is always the same for the same quantity of electricity, whatever may be the intensity indications of the common electrometers placed in connexion with the battery. M. le Professeur Riess says, on the other hand, that this announcement is "untrue," that he has found the heating effect of the discharge inversely proportional to the extent of the battery upon which the electricity is accumulated, that is to say, proportional to the product of the quantity by its "*density*." Let us here pause for a moment to consider what we are really to understand by the term "density" of the electricity accumulated in a battery, and which we imagine to be measured by the ordinary "intensity" electrometers†.

8. If we rigorously examine this very hypothetical question, we shall find that these instruments do not really furnish us with any information whatever relative to "tension" or "density" of the accumulated electricity at the instant of discharge; that is to say, at the instant in which the accumulation coming from every point of the coated glass, falls, as it were, in a concentrated form upon the metallic wire, the subject of experiment; neither do

* The best liquid for ordinary purposes is plain distilled water, coloured by a little tincture of cochineal. Spirit is liable to damage the cemented metallic portions of the instrument.

† "Outre la quantité d'électricité, il importe de connaître sa *densité*, laquelle dépend de l'étendue s de l'armure intérieure de la batterie. . . . . Pour une même quantité d'électricité cette densité est inverse de cette étendue, de sorte qu'on peut l'exprimer par la fraction $\frac{q}{s}$. *La densité peut être déterminée directement au moyen d'un électromètre à poids, &c.*"—De la Rive, *Traité de l'Electricité*, vol. ii. p. 159. The author gives a figure of the balance-electrometer employed by M. Riess, as figured in his work, *Reibungs-elektricität*, vol. i., and by which he infers the density of the charge.

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they discover to us any specific quality of the electrical agency calculated to modify its effects when discharged under the form of a momentary current. All we learn from these instruments is, the relative state of activity of the whole, or a portion of the charge in a certain direction taken in terms of a given statical force, either attractive or repulsive, and by which we may occasionally, and under certain conditions, measure the quantity of electricity accumulated. Now whatever may be the extent of the battery upon which we suppose the charge to be expanded, or whatever may be the distance of discharge as determined by Lane's discharger, in any case at the moment of discharge, when the statical indications often termed "intensity" vanish, and the whole accumulation becomes, as it were, precipitated upon the metallic wire, the force of the momentary current through the circuit is precisely the same, as may be demonstrated by incontrovertible experiments to be presently referred to. The heating effect, therefore, of the discharge must necessarily be independent of any variable indication of an electrometer attached to the battery, and which may be caused at pleasure to indicate with the same quantity of electricity any "density" we please.

9. In my paper of 1830, printed in the Transactions of the Plymouth Institution, as well as in my several communications to the Royal Society, I have shown that the electrometer indications are proportional to the square of the charge divided by the square of the surface or extent of coated glass upon which the accumulated electricity becomes expanded, all other things being unchanged*; so that we have $F = \frac{Q^2}{S^2}$. If, therefore, the heating effect of the discharge be dependent on the extent of the battery, as insisted on by M. Riess, we should have $T = \frac{Q^2}{S^2}$, and not $T = \frac{Q^2}{s}$, as given in his expression. But both these expressions are evidently inapplicable to the heating effect of the discharge, which is altogether independent of S or s taken to represent the extent of the battery; nevertheless I do not doubt the truth of the expression $T = \frac{Q^2}{s}$ when correctly interpreted. I will therefore now endeavour to show in what the difference between my experiments and M. Riess's interpretation of the phænomena consists.

10. It is to be here observed, that when we discharge a given quantity of electricity through a metallic wire, the heating effect will be reciprocally proportional to the resistance in the circuit, that is generally to the extent of the circuit; so that in putting C = the extent of the circuit = the resistance, we have

* Phil. Trans. for 1834, p. 221.

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$T=\frac{1}{C}$, or $T=\frac{1}{r}$. I have shown, for example, in my communications to the Philosophical Transactions*, that with circuits of copper wire varying from 300 to 900 feet in length, arranged in a zigzag form upon insulating supports, the effect of a given quantity of electricity discharged through the wire of the thermo-electrometer is inversely proportional to the length of the circuit, the circuit in this case being taken in lengths of 300, 600, and 900 feet, a result which M. Riess has himself confirmed by a subsequent and similar experiment; hence my expression for the heating effect of the discharge (1) becomes $T=\frac{Q^2}{r}$. Now it is important here to observe, that this expression $T=\frac{Q^2}{r}$ is virtually the same as M. Riess's expression $T=\frac{Q^2}{s}$, the symbol s being, when correctly interpreted, nothing more than the resistance we necessarily introduce into the circuit of discharge, in augmenting the number of charging rods and other obstacles, when we extend our battery by increasing the number of jars; to which we must add the resistance arising from a division of the coated glass upon which the charge is accumulated. In order to make an exact experiment, we should accumulate and discharge the same quantity of electricity, either from a given number of jars of different amount of coated surface, or otherwise from a single jar in which the extent of the coating may be varied, or otherwise from coated glass of variable thickness. In either case we may change the indicated "density," "déterminée directement au moyen d'un électromètre à poids," without changing the resistance in the circuit. In all these cases, however, although the electrometer greatly varies, the heating effect of the discharge remains the same. If M. De la Rive and M. Riess had resorted to experiments of this kind, they would have found the heating effect quite independent of what they term "density" of the electricity in the battery as measured by a balance electrometer, or otherwise estimated by the extent of the battery.

11. Take, for example, two jars, A, B, fig. 8, one having about a square foot and a half of coating, the other five or six square feet, charge them with the same quantity of electricity, and then discharge each in succession through the wire of the thermo-electrometer E, taking care to employ in each case the same charging rods and circuit. The heating effect will be the same, or very nearly so, notwithstanding that the "density," as indicated by the electrometer, may with the small jar A be sixteen

* Trans. Roy. Soc. for 1834, p. 228.

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times as great as the large jar B, and although the discharge can pass over four times the distance as measured by a Lane's electrometer. This experiment alone, without other considerations, appears to me conclusive of the fact, that the hypothesis of a variable "density" in the accumulated electricity has no substantial foundation, at least so far as it relates to the intensity indications of the electrometer.

12. It is certainly true, as laid down by M. De la Rive in the second volume of his recent work*, and as I long since determined†, that the statical force of a given accumulation will, under all circumstances, be in some inverse proportion to the extent of the coated surface upon which the electricity is accumulated; and this is the case whether the increased surface be derived from several jars, or from single jars of different magnitudes, although the precise reciprocal proportion of the surface for the latter case may not probably coincide with that of the first. This is, however, a very different affair to that of the *effect of the discharge* of the accumulated electricity; and M. De la Rive is certainly inexact when, upon the authority of M. Riess, he confounds the operation of the electrometer with "density," and infers that this "density" is in proportion to the number of equal jars of which the battery consists‡. We should really be led into serious errors in our analytical expressions if we assumed the existence of any hypothetical quality such as "density," as referable to the electrometer, the indications of which vary with the extent of the battery upon far different principles.

13. The whole of this question of "density" or "tension," and "intensity," as it is sometimes denominated, referable to a statical electrometer attached to the battery, is quite explicable upon the principles of electrical induction. The active force of any given quantity of electricity accumulated on an insulated conductor will be always apparently diminished by the influence of a second neutral conductor in a perfectly free state, that is, placed in communication with the ground, or otherwise by the influence of a second insulated conductor charged with an opposite electricity. This is really the great secret of the Leyden experiment. In this case the action of the electricity accumulated upon the inner coating is, as it were, masked, or rendered more or less latent by the inductive action of the exterior coating in communication with the earth, or by the influence of the opposite electricity. The force, therefore, in the direction of the electrometer becomes neutralized to a greater or less extent by the influence of the two coatings on each other acting in the direction of the intermediate glass: the indicated intensity will

* Vol. ii. p. 159. † Phil. Trans. for 1834.

‡ *Traité de l'Electricité*, vol. ii. p. 159.

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be hence in some inverse ratio of this inductive action. Here is, in a few words, the reason why we find the intensity of a given quantity of electricity accumulated on thick glass so much greater than that of the same quantity accumulated on thin glass; the coatings are, in fact, in the latter case nearer together, and consequently the action in the direction of the glass in the latter case greater. The tendency of the accumulation is really to break down the glass intermediate between the two coatings. When, however, the opposite electrical forces operate almost entirely through an external circuit joining the two coatings, all the force in the direction of the intermediate glass vanishes, and the whole accumulation being, as it were, thus set free, discharges through the given circuit. The question of the electrometer indication is purely a question of the development of force in one direction rather than in another, and is certainly no sort of measure of an assumed quality of "density" in the accumulated electricity.

14. That the heating effect of the discharge is less as we extend our battery by increasing the number of jars, the quantity of electricity being the same, is also no doubt true; and if M. De la Rive or M. Riess had met with my paper before quoted (1), and other of my philosophical memoirs, he would have found this question fully investigated, as well as several others of which he has so ably treated. I have shown, for example,—

(1.) That the heating effect of the discharge is proportional to the square of the accumulation, all other things being the same*.

(2.) That the heating effect is diminished when we accumulate and discharge a given quantity of electricity from a divided surface†.

(3.) That the heat excited in a metallic wire is reciprocally proportional to the length of the circuit of discharge or resistance, that is to say, to the retardation or duration of the discharge‡.

(4.) That by the introduction of imperfect conductors into the circuit, such as water contained in tubes of glass, the heating effect becomes extremely small§.

The expression $T = \frac{Q^2}{r}$, putting $r =$ the retardation, and which is really nothing more than the expression $T = \frac{Q^2}{s}$ of M. Riess, has been therefore derived as well from my original investigations, as from the more recent researches of Professor Riess, quoted by M. De la Rive.

* Trans. of the Plym. Inst. 1830, pp. 68, 84. † Ibid.
‡ Phil. Trans. for 1834.
§ Ibid. p. 227, 228. See also Trans. Roy. Soc. Edinb. vol. xii.

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(5.) I have shown, that, under certain conditions, the best conductor may become the most heated, because it can transmit a greater quantity of electricity*.

15. In the Philosophical Transactions for 1827 will be found an experimental investigation of the relative conducting powers of different metals. The results are in accordance with the best general deductions of both the old and modern electricians, distinguished by their inquiries in this department of physics. It will be seen in this paper last referred to, that the heating effect of the electrical discharge on a metallic wire of a given diameter is precisely the same as that upon four wires of half the diameter and of equal length; that is to say, in elongating the wire to four times its length by the ordinary mechanical means, and placing it in the ball of the thermometer under the form of four small wires.

Now in this experiment we may perceive, that since the diameter of each of the smaller wires is one-half the diameter of the large wire, and that each of the smaller wires, in transmitting one-fourth part of the charge, contributes one fourth-part of the total effect, it follows, that if the whole charge were transmitted by one of the smaller wires singly, the heating effect on that wire would become sixteen times as great, since it would transmit four times the quantity of electricity, the heating effect being as the square of the quantity (14). Let, for example, the total effect upon the larger wire, or upon the four smaller wires, be 16 degrees of the thermometer scale. In this case we have 4 degrees for each of the small wires considered alone. Now in discharging all the electricity through one of these small wires, we should have a heating effect equal to 64°; that is to say, four times the effect on the original or large wire. The comparative heating effects, therefore, on these two wires are in the inverse ratio of 1 : 4, whilst their respective diameters are directly as 1 : 2. The heating effects, therefore, are reciprocally proportional to the squares of their diameters or to the squares of their radii, that is, inversely as the area of the section. Here is again another result of the practical application of my instrument such as I constructed it, quite in accordance with well-known laws of conduction since determined, as also in all the subsequent researches of M. Riess.

16. I might quote many other examples, all confirming the accuracy of my instrument as an instrument of quantitative electrical research, such as I constructed it, and its singular operation in producing results which are now received as general laws of electricity. How then can it be said, with any degree of justice, as announced by M. De la Rive on the authority of M. Riess,

* Edinb. Phil. Trans. vol. xii.

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that the construction of my instrument is imperfect*, and "that is the reason why my experiments have been unfruitful," more especially when we see that almost every experimental deduction arrived at by M. Riess, by means of what he considers a better construction, is in perfect accordance with results I had already obtained. I trust I have clearly shown (10) that the solitary exception taken by M. Riess in his expression $T\alpha\frac{Q^2}{s}$ is, after all, no exception at all; the expression being, in fact, no other than my own formula $T\alpha\frac{Q^2}{r}$ previously deduced, since the symbols s and r may be taken to represent the same thing, viz. resistance to discharge.

17. Since the difference in question bears upon a large and important class of physical researches, I am unwilling to leave the subject without some few additional observations. My first experiments, given in the volume of the Plymouth Institution before mentioned (1), contain illustrations of the diminished heating effect of a given quantity of electricity when accumulated on, and discharged from, many jars—the only point, in fact, upon which M. Riess founds his objection to my instrument in the way I constructed it. It will be found at page 16 of this paper, that the quantity of electricity was measured in precisely the same way as that subsequently adopted by M. Riess, many years after, that is, by the explosions of a Lane's jar in communication with the insulated negative side of the battery, although M. Riess, full seven years after (Poggendorff's *Annalen*, vol. xl. p. 324), claims for himself the especial merit of having first applied this practically. If M. Riess, however, will favour me by a perusal of my memoir above referred to (1), he will find the whole arrangement exactly as he describes it, fully detailed and figured, p. 63, fig. 16, printed in 1830. The method, however, which I finally adopted to measure the quantity of electricity in the battery, as being the most accurate and convenient, was the interposition of a small Leyden phial between the conductor of the machine and the battery, and which I have termed a "unit jar" or measure. It is fully described in the Philosophical Transactions for 1834†. The electric jars first subjected to experiment were part of a battery originally constructed by Cuthbertson, each jar being 18 inches high and 4 inches in dia-

* "It is probably to the imperfection of the one (thermo-electrometer) employed by Mr. Harris that we must attribute the inaccuracy of his conclusions."—De la Rive, vol. ii. p. 215. English translation.

† Page 217. Nothing can exceed the accuracy of this measure, although M. Riess labours to show the contrary.

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meter, containing about a square foot and a half of coating, as A, fig. 8.

18. The results of a series of experiments with six jars clearly showed that the same quantity of electricity discharged from several jars combined, has not so great a heating effect as when discharged from a single jar, or from a less number; the effect, in fact, continued to diminish in some inverse ratio of the number of jars: this is, in fact, Prof. Riess's experiment. With a view of ascertaining how far this result depended upon an extension of the battery in coated surface, I proceeded to charge a single large jar, equal in surface to three or more of the first jars taken together, and with the same quantity of electricity as at B, fig. 8. Now in this case the battery was extended, not by a *divided*, but *by a continuous* surface. The heating effect was now the same as when the same quantity of electricity was discharged from a single jar, not exposing above one-fourth the surface, as already observed (10), notwithstanding that the relative "*tensions*" or "densities" indicated by the electrometer were nearly as 16 : 1. The result in question, therefore (17), as shown in the experiment before quoted (11), could not possibly depend upon any hypothetical change in the "density" of the accumulated electricity, but must necessarily arise out of some disturbing force tending to weaken the power of the current of discharge, which disturbing force could be no other than the resistance introduced into the circuit by the extension of the battery in added jars. It is quite impossible, as already observed (10), to extend our battery in this way without at the same time increasing the resistance to discharge by the added rods requisite to charge and discharge the whole combination. And this resistance is still further increased by the use of small metallic chains, often employed to transmit the electricity to the inner coating.

19. Here it is also most important to observe, that in the ordinary electrical battery there is always some resistance to discharge, arising out of the translation, as it were, of the electricity accumulated on the surface of the glass to the coating, by the conducting power of which the electricity is collected from every point of the glass, and transmitted through the circuit of discharge. Now in the common construction of the electrical jar, the coating is never so closely applied to the glass as to become as it were identified with it, and so effect this operation perfectly. In the batteries as constructed by the old electricians, a thin sheet of paper was often interposed between the coating and the glass, with a view of avoiding fracture. In this case the resistance to the free translation of the electricity through the coating is remarkable. In the course of my experiments with the jars of the battery before mentioned (16), I found one of the

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jars so very different in its action from the others, that I was led to strip off the coating in order to examine the precise condition of the surface beneath. Having done this, I found a thin sheet of paper pasted upon the surface of the glass, which being an imperfect conductor became a source of obstruction in the circuit. When this paper was displaced, and the coating applied immediately to the glass, the jar acted in every respect like the others. Every kind of cement, therefore, employed to attach the coating to the glass would cause some resistance to the free translation of the charge, according as it is more or less insulating, or of greater or less thickness; when consisting of any resinous substance, such as common bees-wax, the increased resistance is very considerable.

20. If we coat a jar with an imperfect conductor, such as water, as in the original experiments of the Germans and Dutch, the resistance to discharge is especially marked. In this case the heating effect of a given quantity of electricity accumulated and discharged from such a jar is almost inappreciable by the thermo-electrometer as commonly employed; so that if the original experiment had been perpetuated under the form first given by the Leyden experimentalists, we should have known very little of the heating effects of the ordinary electrical battery on metallic wires. I recently gave a jar 30 inches high and 10 inches in diameter, a coating of 5 square feet of water, the uncoated interval being carefully varnished, and charged it with a measured quantity of electricity. The indicated "intensity" or "density," according to M. Riess, of the charge, as measured by a statical electrometer, was nearly the same as that of a similar jar coated with metal and charged with the same quantity of electricity. Still the heating effect of the discharge from the water-coating was scarcely appreciable by the thermo-electrometer then employed; it was certainly not the one-thirtieth part of the effect of the discharge of the same quantity of electricity from the metallic coating, although on introducing a tube of water into the circuit, the discharge readily set fire to inflammable matter such as gunpowder.

21. We find therefore always some resistance to discharge, arising out of the necessary construction of the jar itself, a resistance altogether independent of the resistance proper to the extent of the circuit of discharge, and which it is requisite to consider, and add as a constant whenever we desire to calculate the total resistance. If, for example, we would seek to discover the comparative resistance of metallic circuits varying in length, we must add to the resistance of each circuit this constant resistance in the battery itself, more especially if the quantity of electricity be small and the given circuits of small extent. It is

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only when we employ large quantities of electricity and circuits of considerable extent, that we may neglect the battery resistance as being extremely small. It was not until I employed circuits of 300 to 900 feet long (10), and a considerable electrical accumulation, that I found the numbers representing the heating effect on the thermo-electrometer in the simple inverse ratio of the length of the circuit, or nearly so. This source of resistance to discharge, therefore, is an element of much importance. It appears however to have escaped M. Riess's attention altogether.

When we take into account these several sources of disturbance, we can scarcely hope to find the heating effect of a given quantity of electricity always reciprocally proportional to the number of jars, or what M. Riess calls the "extent of the battery," although the results may approach that ratio.

In some instances, if the quantity of electricity be small and the jars of the battery of great capacity, having for example from 4 to 6 square feet of coated glass each, then the comparative resistance introduced into the circuit of discharge by the addition of other similar jars, becomes of much greater importance; if, however, we increase the quantity of electricity, the comparative influence of this resistance will be diminished. I found, in accumulating a large quantity of electricity upon jars of great capacity, that the effect was not greatly different while the accumulations were effected upon one jar or two; but this result did not obtain with a small charge.

22. The announcement, which I first made in 1830, of the law of electrical discharge, the subject of this paper, must be understood rather in relation to the indications of the ordinary statical electrometers attached to the battery, than in respect of any hypothetical condition of the discharge itself as to "tension" or "density," as announced by M. le Professeur Riess. All I pretend to state is, that the heating effect is altogether independent of the extent of surface upon which the electricity is expanded, and of all electrometric indications, all other things being the same.

23. With respect to my experiments, they are certainly not open to the criticism with which M. Riess, quoted by M. De la Rive, has regarded them. They were made with great care, and no expense was spared in the construction of the electrical apparatus. The electrical machine employed was a most perfect instrument. It had a plate of glass 3 feet in diameter, and was well adjusted in all its parts; the cushions were insulated on each side of the plate, and were joined by an efficient negative conductor. The action of this instrument, when in working condition, was perfectly regular and efficient, and produced a precisely equal quantity of electricity at each turn of the plate.

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The battery consisted of five jars of similar dimensions, B, fig. 8, each containing 5 square feet of coated glass. When fully charged, it readily melted and fused into balls 15 feet of fine iron wire.

The intensity of the accumulation was valued by means of a statical electrometer of great accuracy, the action of which depended on the attractive force directly exerted between two small circular planes, and reducible to a known standard of weight.

The platinum wire employed in the thermo-electrometer to measure the heating effect was of sufficient diameter to completely transmit the whole of the charge. I was not ignorant of the precautions necessary to be observed in this respect (13), and I have fully considered them in my memoir in the 12th vol. of the Edinburgh Philosophical Transactions.

In estimating the quantity of electricity accumulated, I resorted to three different methods,—1st, the revolutions of the plate of the machine as indicated by a divided circle, and an index fixed on the axis of the plate; 2ndly, by insulating the battery in the way already mentioned (17); 3rdly, by the "unit jar," also before mentioned (18).

24. In reply to the remarks (Poggendorff's *Annalen* for 1841, vol. lii. p. 318) that "I have no clear idea of the theory of the instrument I employed," &c., I have to observe, that I did not think it requisite in these experiments to consider the first temperature of the wire, the specific heat of the air in the ball of the thermometer, and other small elements of that kind, so perfectly calculated by M. Riess in his formula

$$T=\left(\frac{\iota}{\alpha}+t\right)\left(\frac{\cos\phi}{nb}+\frac{\iota}{\nu}\right)\left(\frac{mc}{MC}+1\right)\theta.$$

Any correction which might arise out of such elements in the results of the experiments in the way I conducted them would be extremely small, and much less than the errors of observation inseparable from the experiment itself. Indeed we deceive ourselves greatly in physical inquiries, when we attempt to reach a degree of refinement inconsistent with the kind of experiments in which we are engaged. It often serves only to complicate the calculation, and introduce new sources of error into our experimental deductions. The first terms of M. Riess's formula, just quoted, could have no relation to my method of manipulation. With respect to the last, I have to observe, that if we attentively consider the nature and mode of operation of the instrument itself, we shall at once perceive that its indications depend on the momentary expansion of a small cylindrical column of air immediately in contact with, and surrounding the

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fine wire *pn*, fig. 5, passing through the thermometer-ball; an impulsive movement thus becomes mechanically communicated, as it were, to the general mass, which, pressing by its elasticity on the surface of the fluid in the reservoir beneath, causes the fluid to ascend along the scale of the instrument. The current of discharge in its momentary passage through the wire undoubtedly excites in it a greater or less degree of heat. Still the effect is very evanescent, and, as it appears to me, there is not the least ground for concluding that the entire mass of the air in the thermometer-ball experiences an elevation of temperature; to effect this, some short, but still very sensible portion of time would be requisite, but little or no time elapses. No sooner has the electrical discharge passed through the wire, than the wire appears instantly to recover its original temperature. The thermometer-fluid, which at the instant rapidly ascends the scale, as rapidly and immediately descends, and not unfrequently sinks below the zero-point from which it started, a phænomenon quite inconsistent with the notion that the temperature of the mass of the air in the ball had been permanently elevated, and which if so elevated would necessarily demand some time to cool down again to its previous point. How the doctrines of specific heat can be well applied to such an action as this is not by any means clear. Heat is certainly not added to the mass of air in the ball, or even to the wire in the ordinary way. The heat excited in the wire appears to be the result of a momentary mechanical action, just as we render a nail red-hot by a few blows of a hammer; but however this may be, we can scarcely venture, in the present imperfect state of our knowledge of the causes of heat and electricity, to apply abstract theoretical formulæ to the indications of such an instrument, the precise value of which, as a measure of a certain species of electrical force, we can only arrive at empirically by experiment; and after all we must not take the instrument for more than it is worth.

25. Experimentally, however, we find a marked accordance between the degrees of movement of the fluid along the scale, and well-established laws of electricity. Take, for example, the well-established law expressed in the formula $F=Q^2$, anticipated by Cuthbertson, and first verified by myself in 1830 (14), although not referred to by M. De la Rive at p. 146, tom. ii. of his recent work: here we observe that if twice the quantity of electricity be accumulated and discharged through the wire, the fluid ascends to four times the height; three times the quantity causes it to reach nine times the altitude. Then, again, take the relative conducting powers of various metals. If, as is pretty well ascertained, we take the heating effect in the simple inverse ratio of the conducting power, we find equal wires of

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different metals introduced into the ball of the thermometer evince heating effects in this same reciprocal proportion. Thus in the ordinary case of the electrical discharge, silver and copper are with a given quantity of electricity the least heated, and lead the most; gold, zinc, platinum, iron, tin, &c. come in due place between these extremes; whilst the degree of effect indicated on the scale taken inversely, approximate very closely to the comparative values of these relative conducting powers. It is especially remarkable that the conducting powers of copper and lead, as thus determined, have precisely the same relative value as given in the numbers of M. Becquerel*. The numbers for other metals do not differ considerably when we take into the account the great variety of circumstances liable to derange the result. These, and other experimental facts which might be quoted, favour the conclusion, that the simple degree of movement of the fluid in the stem of the instrument, without further correction, is after all the best measure of the force of the current in the wire; it at least furnishes approximations sufficiently near as to leave no doubt on the mind of the laws we seek to discover. All we require, therefore, in the use of this instrument is a careful manipulation, and due attention to the dimensions of the wire in the ball, and other conditions of the experiment. Taking these several facts, to which I have called attention, into consideration, I am not disposed to allow the justness of M. Riess's criticisms on my original inquiries, or that the course of experiment pursued by Prof. Riess, and to which M. De la Rive devotes so large an amount of consideration, is so perfect as that which he condemns.

26. It is not without regret that I observe M. Riess's systematic disparagement of what I have effected at various times in this department of science. Until my several papers first appeared, we had really few, if any, available quantitative processes in electricity. In the course of these papers I was the first to point out and furnish practical methods of quantitative measurement, and illustrate thereby many important laws of electrical action. Many of these processes have been virtually adopted by others. "L'électromètre à poids," figured at p. 160, vol. ii. of M. De la Rive's work, together with the process he there describes, is really a bad adaptation of my electrical balance described in the Philosophical Transactions for 1834, and it will be found practically inaccurate. The exceptions taken by M. Riess to my several instruments and my methods of research are without any good foundation whatever. In alluding to my "unit measure," (Poggendorff's *Annalen*, vol. xl. p. 323), M. Riess observes, "the measure of electricity by the revolutions of the electrical machine

* *Traité expérimentale*, tom. iii. p. 91.

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only furnishes a rough, an inexact measure of the quantity of electricity".... "The method employed by Harris in the Philosophical Transactions for 1834 is still worse." Losing sight of all the novelty and philosophy of my simple and useful little instrument, M. Riess treats it as a mere casual employment of a Lane's bottle, and then proceeds to apply to it some rather commonplace and unsound objections. Yet in following out my arrangement of an insulated battery with a Lane's electrometer jar in connexion with the outer coating (17), M. Riess really compromises his own principles. If, as he states (Poggendorff's *Annalen*, vol. xl. p. 323), "a bottle is more completely charged when there is no obstruction to the action of the outer coating," then if my unit jar be inaccurate on this ground, surely the Lane's bottle dignified in M. De la Rive's work with the title of "Bouteille électrométrique," directly interposed between the outer coating of the battery and the earth, must be necessarily at least an equal obstruction to the charging of the battery. The fact is, that there is little or no obstruction at all in either case. The unit jar neutralizes at each explosion, and each discharge must correspond to an equal quantity of electricity accumulated in it. This must be so on the principle long since established and admitted by M. Riess himself, viz. that the quantity of electricity accumulated in a jar will be as the distance of the discharge directly, all other things being the same. Now submit the unit jar to this experimental truth. Attach a Lane's electrometer to a jar exposing about five square feet of coating, or to a battery of smaller jars; set the balls to given measured distances, say to successive distances which are to each other as 1:2. Here it will be found, if the experiment be carefully made, and there is no dissipation of the charge, that at twice the distance twice the number of units as measured by the jar will correspond with the great explosion of the large jar. But how could this happen if the quantity represented by twice the number of discharges of the unit measure was not double the quantity represented by the number of explosions or discharges taken as unity? Faraday, one of the best authorities on such questions, has, upon a full consideration of the subject, acknowledged the accuracy of my views*. So far from this method being open to the doubts which M. Riess has thrown upon it, it will really be found much more accurate than the method of insulating the battery, which is only a clumsy way of effecting the same thing. The large open surface of an insulated battery is always liable to give off electricity in other directions than that of the "bottle of measure," and we are always at the mercy of the insulations.

27. If we turn to Poggendorff's *Annalen*, vol. lii. for 1841,

* Noad, Manual of Electricity, p. 141.

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p. 315, there we find a somewhat laborious and learned endeavour by M. Riess to confound my thermo-electrometer, figs. 1, 2, 3, &c., with the old air electrometers of Kinnersley and Beccaria. My instrument is treated as a mere extension of these instruments. I have merely the "merit" of suggesting the present application of them; but I think anyone who at all dispassionately considers the nature and construction of my thermo-electrometer, will see that the refined instrument described in the Philosophical Transactions for 1827 is really no copy whatever of the old air electrometers. I had certainly not the least idea in my mind of such instruments when I contrived it, and which I did to satisfy the Scientific Commission appointed by the Admiralty in 1823 to examine my proposals for giving effectual security to the Royal Navy from lightning. It was important to me at that time that I should exemplify, by original researches, the relative conducting powers of various metals. Men of no less scientific standing than Sir H. Davy and Dr. Wollaston examined my experimental inquiries, and honoured them with their approbation. My instrument was subsequently submitted to the Royal Society by Sir H. Davy without the most distant idea of its being a mere copy of the old air electrometers by Beccaria and Kinnersley. Fig. 9 represents one of these instruments by Beccaria, from which M. Riess would have it inferred that mine was derived. But whether we take this or the air instrument of Kinnersley, they neither of them were contrived to do more than illustrate the mechanical force of an electrical explosion in a confined space of air, and I cannot but regard it as a great misapprehension in M. Riess when he identifies my thermo-electrometer with such instruments. Moreover, I cannot admit any common association with M. Riess in the first application of the principle on which my instrument depends, as expressed in the second volume of M. De la Rive's work, p. 154; and I regard the arrangement figured, p. 156, as nothing more than a similar arrangement of my own, fig. 3 of this paper, which I employed at least fifteen years before M. Riess's papers appeared in Poggendorff's *Annalen*. With respect to the accuracy of my researches, I am quite prepared to test them by sound philosophical evidence. It is always easy for a learned and able writer to deal severely with the researches of others, and undervalue claims to originality in the invention of philosophical instruments, more especially when such claims and researches are immediately in his own path: it is a course by no means uncommon in the history of physical science; but it is not perhaps so easy to defend such a course, however unpremeditated, upon just, liberal, and enlightened grounds.

28. I am unwilling to conclude these observations without

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proper acknowledgement of my sense of the value of Professor Riess's many interesting researches in this branch of physics. It is really with no view to a painful philosophical controversy that I have been led to submit this paper for the consideration of the scientific world, but solely with a view to a correct interpretation of very important electrical phænomena and the progress of electricity. I would also, as already observed, desire to obtain some little consideration, in justice to myself, in reply to the rather disparaging criticisms which M. Riess has been led to make on my original researches, and which have been further and recently repeated by M. De la Rive; and I especially invite the attention of those engaged in electrical investigations to my several papers referred to in this memoir.

Plymouth, April 5, 1856.

XLIV. *On certain Modifications of the Form of the new Double-acting Air-pump with a Single Cylinder.* *By* T. Tate, *F.R.A.S.**

THE characteristic feature of the new air-pump, described in the Philosophical Magazine for April 1856, consists in the double piston acting in a single cylinder. For the sake of distinction, I shall call this form of the pump No. 1. I have since constructed this pump with different systems of valves, with the view of determining the form which is most eligible for general use.

There is a little loss of dynamic effect in working the pump No. 1, from the circumstance that one of the solid pistons forms a vacuum on one end of the cylinder at every stroke; but this loss of *work*, or dynamic effect, is considerably less than that which takes place in the common pump. In order to eliminate this defect, I have placed valves in the pistons A and B, lifting towards the corresponding ends of the cylinder. By this arrangement the exhaustion is performed with the least possible expenditure of work or dynamic effect. This form of the pump, for the sake of reference, I shall call No. 2. But this œconomic construction is attended with a little loss of exhausting power, owing to the greater amount of air which fills the valve spaces. Whilst the exhaustion effected by No. 1 is measured by about two-tenths of an inch of mercury, that of No. 2 is measured by about four-tenths of an inch.

Some persons, in my opinion without a sufficient reason, seem to object to the piston passing the exhaustion orifice E. In order to suit the views of such persons, I have constructed another form of the pump, which I shall call No. 3, in which the

* Communicated by the Author.

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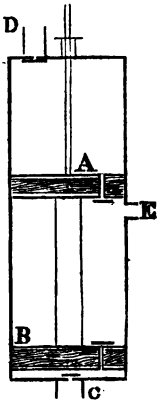
valves are constructed after the manner of No. 2, but in which the pistons are placed a little more apart, so that neither of them ever passes the exhaustion orifice E, but at the end of every stroke the interior face of one of the pistons just arrives at the corresponding edge of the exhausting orifice. It is scarcely necessary to observe, that this form of the instrument possesses the same property as that of No. 2 with respect to the œconomy of dynamic effect; but it will be seen that its exhausting power is the same as that of the common pump.



With the view of showing the applicability of the advantages of the double piston acting in a single cylinder to the valve system of the common pump, I have constructed the form, which I shall call No. 4, represented in the annexed cut, where the arrangement of the pistons, A and B, is the same as in No. 3. In this case all the valves lift inwards; the orifice E, at the centre of the cylinder, leads into the atmosphere; and the pipes C and D lead to the receiver. The exhausting power, as well as the dynamic effect, of this pump is the same as that of an ordinary double-barrelled pump. At the same time it must be observed, that this form of the double piston acting in the single cylinder, enables us to apply the moving pressure in the most simple manner.

I have found the friction of a single piston, $1\frac{1}{2}$ inch in diameter, to be $2\frac{1}{4}$ lbs., and that of the double piston with the stuffing-box of the piston-rod to be $3\frac{1}{4}$ lbs.; with these experimental data I have found the work applied to the different pumps necessary to exhaust the air from a receiver containing 108 cubic inches, to be as follows:—

The *work* of No. 1 (requisite to produce the given exhaustion) is nearly *one-half* that of the common pump.

The work of Nos. 2 and 3 is nearly *one-third* that of the common pump.

Taking all circumstances into consideration, I am persuaded, that, for most purposes, the pump with the solid pistons (No. 1) is the most advantageous form of the instrument.

The following is an investigation of the formulæ employed in calculating the work expended in exhausting the receivers of the different pumps.

1. To find the work expended in exhausting the receiver of the common air-pump.

Let U_n = the work in the nth double stroke; u = the work expended in overcoming the pressure E of the external air; u_1 =

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the work expended in friction, f being the friction on the piston supposed to be one inch in the section; u_2 = the work performed by the air in the pump upon expanding from the pressure E_{n-1} to E_n; u_3 = the work accumulated in the air in its transfer; then

$$U_n = u + u_1 - u_2 + u_3.$$

Here $u = \frac{bE}{12}$; $u_1 = \frac{2fb}{12}$; $u_2 = \frac{1}{12} a E_{n-1} \log_e \frac{E_{n-1}}{E_n}$

$= \frac{1}{12} a \left(\frac{a}{a+b}\right)^{n-1} E \log_e \frac{a+b}{a}$, for $E_n = \left(\frac{a}{a+b}\right)^n E$ when the resistances of the valves are neglected; $u_3 = \frac{b}{1728} \cdot \frac{wv^2}{2g}$, where w is the weight of a cubic foot of atmospheric air, and v the velocity of discharge, the *vis viva* of the air in passing through the lower valve being neglected;

$$\therefore \Sigma_{n=1}^{n} U_n = \Sigma_{n=1}^{n} \frac{1}{12} \left\{ b(E+2f) - a\left(\frac{a}{a+b}\right)^{n-1} E \log_e \frac{a+b}{a} + \frac{b}{144} \cdot \frac{wv^2}{2g} \right\}$$

$$= \frac{1}{12} \left[nb(E+2f) - \frac{a(a+b)}{b} E \left\{ 1 - \left(\frac{a}{a+b}\right)^n \right\} \log_e \frac{a+b}{a} + \frac{nb}{144} \cdot \frac{wv^2}{2g} \right]. \quad (1)$$

And when n is very great, we find the work expended in exhausting the receiver

$$= \frac{1}{12} \left\{ nb(E+2f) - \frac{a(a+b)E}{b} \log_e \frac{a+b}{a} + \frac{nb}{144} \cdot \frac{wv^2}{2g} \right\}. \quad (2)$$

2. To find the work expended in exhausting the receiver of the pump No. 1.

In this case we have for the work of the nth single stroke,

$$U_n = u + u_1 + u_2 + u_3.$$

Here $u = \frac{1}{12} a' E$, where a' is the volume of the air in the barrel when its pressure becomes E, but $a' = \frac{bE_{n-1}}{E}$;

$$\therefore u = \frac{b}{12} \left(\frac{a}{a+b}\right)^{n-1} E; \quad u_1 = \frac{1}{12} fb;$$

$$u_2 = \frac{1}{12} a' E \log_e \frac{E}{E_{n-1}} = \frac{1}{12} (n-1) b \left(\frac{a}{a+b}\right)^{n-1} E \log_e \frac{a+b}{a};$$

$$u_3 = \frac{a'}{1728} \cdot \frac{wv^2}{2g} = \frac{b}{1728} \cdot \frac{wv^2}{2g} \left(\frac{a}{a+b}\right)^{n-1};$$

$$\therefore \Sigma_{n=1}^{n} U_n = \Sigma_{n=1}^{n} \frac{b}{12} \left[\left(E + \frac{1}{144} \cdot \frac{wv^2}{2g}\right) \left(\frac{a}{a+b}\right)^{n-1} + (n-1)\left(\frac{a}{a+b}\right)^{n-1} E \log_e \frac{a+b}{a} + f \right];$$

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and when n is taken very great, we find the work expended in exhausting the receiver

$$= \frac{a+b}{12}\left\{\mathrm{E}\left(1+\frac{a}{b}\log_e\frac{a+b}{a}\right)+\frac{1}{144}\cdot\frac{wv^2}{2g}\right\}+\frac{1}{12}nfb. \quad . \quad (3)$$

It will be observed, that the work in passing the exhausting orifice has been neglected in this investigation.

3. To find the work expended in exhausting the receiver of the pump No. 3, and also No. 2 very nearly.

In this case we have for the work of the nth single stroke,

$$U_n = u + u_1 + u_2 + u_3.$$

Here u, u_1, and u_3 have the same values as in the preceding case, and

$$u_2 = \frac{1}{12}a'\mathrm{E}\log_e\frac{\mathrm{E}}{\mathrm{E}_{n-1}} - \frac{1}{12}a\,\mathrm{E}_{n-1}\log_e\frac{\mathrm{E}_{n-1}}{\mathrm{E}_n}$$

$$= \frac{1}{12}(n-1)b\mathrm{E}\left(\frac{a}{a+b}\right)^{n-1}\log_e\frac{a+b}{a} - \frac{1}{12}a\mathrm{E}\left(\frac{a}{a+b}\right)^{n-1}\log_e\frac{a+b}{a};$$

$$\therefore \Sigma_{n=1}^{n}U_n = \Sigma_{n=1}^{n}\frac{b}{12}\left[\left\{\mathrm{E}+\frac{1}{144}\cdot\frac{wv^2}{2g}-\frac{a\mathrm{E}}{b}\log_e\frac{a+b}{a}\right\}\left(\frac{a}{a+b}\right)^{n-1}\right.$$

$$\left.+(n-1)\mathrm{E}\left(\frac{a}{a+b}\right)^{n-1}\log_e\frac{a+b}{a}+f\right].$$

And when n is very great, we find the work expended in exhausting the receiver

$$= \frac{a+b}{12}\left(\mathrm{E}+\frac{1}{144}\cdot\frac{wv^2}{2g}\right)+\frac{1}{12}nfb. \quad . \quad . \quad . \quad (4)$$

Now $\frac{(a+b)\mathrm{E}}{12}$ exactly expresses the least possible work requisite for completely exhausting the space $a+b$ of air, and the other parts of the expression give the work of resistances essentially connected with all air-pump pistons; hence it follows, that with this construction of pump, the exhaustion is performed with the least possible expenditure of work.

Let $a=108$, $b=12$, $\mathrm{E}=15$, $w=\cdot086$, $v=100$, $f=2\frac{1}{2}\div\frac{5}{2}=1\frac{1}{2}$ for the single piston, and $3\frac{1}{2}\div\frac{5}{2}=2\frac{1}{2}$ for the double piston, and $n=60$; then by formulæ (2), (3), and (4), we find the work in each case as follows, viz. 950, 440, and 300.

Hounslow, April 18, 1856.

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XLV. *On the Solution of certain Differential Equations.* By Benjamin Williamson, *Fellow of Trinity College, Dublin**.

In "A General Method in Analysis," published in the Transactions of the Royal Society for the year 1844, Professor Boole proposes a method for the reduction of differential equations to others already soluble, and gives several examples of its application.

The object of the present paper is to exhibit some of Professor Boole's results in another form, to apply the same method to another class of differential equations, and to extend such solutions to certain analogous partial differential equations. In doing so, I will restrict myself to the consideration of the different classes of equations which depend for their solution on $(\mathrm{D}^2 \pm a^2)y = 0$.

I. I will commence with the consideration of the equation

$$\left(\mathrm{D}^2 - \frac{2n}{x}\mathrm{D} + a^2\right)y = 0,$$

where D stands for $\frac{d}{dx}$. This equation is at once transformed into

$$\left(x\mathrm{D}.(x\mathrm{D} - 2\overline{n+1}) + a^2x^2\right)y = 0.$$

Assume, in accordance with Dr. Boole's method,

$$y = (x\mathrm{D} - 1).(x\mathrm{D} - 3) \ldots . (x\mathrm{D} - 2\overline{n-1})y';$$

then, since

$$x^2(x\mathrm{D} - 1) \ldots . (x\mathrm{D} - 2\overline{n-1})y' = (x\mathrm{D} - 3) \ldots (x\mathrm{D} - 2\overline{n+1})x^2y',$$

the proposed equation becomes

$$(x\mathrm{D} - 3) \ldots . (x\mathrm{D} - 2\overline{n-1})(x\mathrm{D} \,.\, \overline{x\mathrm{D} - 1} \,. + a^2x^2)y' = 0,$$

or $(\mathrm{D}^2 + a^2)y' = 0$, the solution of which is $y' = \mathrm{C}\cos(ax + \alpha)$. Accordingly that of the proposed equation is

$$\begin{aligned} y &= c.(x\mathrm{D} + 2\overline{n-1}) \ldots (x\mathrm{D} - 3)(x\mathrm{D} - 1)\cos(ax + \alpha) \\ &= c.\left(a\frac{d}{da} - 2\overline{n-1}\right) \ldots \left(a\frac{d}{da} - 3\right)\left(a\frac{d}{da} - 1\right)\cos(ax + \alpha) \\ &= \mathrm{A}\left(\frac{d}{da}a^{-1}\right)^n.\cos(ax + \alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1) \end{aligned}$$

$$\left[\text{since } \left(a\frac{d}{da} - m\right) = a^m\left(a\frac{d}{da}\right).a^{-m}\right].$$

Ex. 2. Let $$\left(\mathrm{D}^2 - \frac{n.n+1}{x^2} + a^2\right)y = 0.$$

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This equation is equivalent to

$$\left((xD+n)(xD-\overline{n+1})+a^2x^2\right)y=0,$$

or

$$\left(xD\,(xD-2\overline{n+1})+a^2x^2\right)x^n y=0.$$

Consequently its solution is by the last

$$y=Ax^{-n}\left(\frac{d}{da}a^{-1}\right)^n.\cos(ax+\alpha). \quad . \quad . \quad . \quad . \quad (2)$$

Ex. 3. $$\left(D^2+\frac{2(n+1)}{x}D+a^2\right)y=0,$$

or

$$\left(xD\,(xD+2\overline{n+1})+a^2x^2\right)y=0;$$

or, as it can be otherwise written,

$$\left(xD\,(xD-2\overline{n+1})+a^2x^2\right)x^{2n+1}y=0.$$

Accordingly the solution is

$$y=Ax^{-2n+1}.\left(\frac{d}{da}a^{-1}\right)^n.\cos(ax+\alpha). \quad . \quad . \quad (3)$$

Ex. 4. If we had substituted for y, $fx.u$ instead of x^nu in *Ex.* 2, the equation would have taken the form

$$\left(D^2fx-\left(\frac{n.n+1}{x^2}-a^2\right)fx\right)u=0,$$

or

$$\left(D^2+2\frac{f'x}{fx}D+\frac{f''x}{fx}-\frac{n.n+1}{x^2}+a^2\right)u=0,$$

its solution being

$$u=\frac{A}{x^nfx}\cdot\left(\frac{d}{da}a^{-1}\right)^n\cos(ax+\alpha). \quad . \quad . \quad . \quad . \quad (4)$$

If we make $\frac{f'x}{fx}=\psi'x$, or $fx=e^{\psi x}$, this equation is immediately seen to be identical with that treated by Dr. Hargreave in the Philosophical Transactions for 1848, and since discussed by Mr. A. H. Curtis in the Cambridge and Dublin Mathematical Journal for the year 1854.

II. In any equation if x be changed into $\frac{1}{z}$, the operation $x\frac{d}{dx}$ becomes $-z\frac{d}{dz}$; and accordingly, if the solution of any equation of the form $\phi(xD)y=\chi$ be known, we can immediately determine that of the analogous equation $\phi(-xD)y=\chi'$. I will illustrate the use of this method of transformation by the solution of a few well-known equations, and then proceed to apply it to the differential equations analogous to those I have already solved.

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Ex. 1. $$\left(D^2+\frac{2}{x}D+\frac{a^2}{x^4}\right)y=0,$$

or $$\left(xD\,.\,(xD+1)+\frac{a^2}{x^2}\right)y=0;$$

let $x=\frac{1}{z}$, then the transformed equation is

$$\left(z\frac{d}{dz}\cdot\left(z\frac{d}{dz}-1\right)+a^2z^2\right)y=0;$$

$\therefore$ the solution is

$$y=A\cos(az+\alpha)=A\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad (5)$$

Ex. 2. $$\left(D^2+\frac{a^2}{x^4}\right)y=0,$$

or $$\left(xD\,.\,(xD-1)+\frac{a^2}{x^2}\right)y=0.$$

The transformed equation is

$$\left(z\frac{d}{dz}\cdot\left(z\frac{d}{dz}+1\right)+a^2z^2\right)y=0.$$

The solution of this is immediately seen to be

$$y=Az^{-1}\cos(az+\alpha).$$

Consequently the solution of the proposed is

$$y=Ax\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Ex. 3. $$\left(D^2-\frac{2n}{x}D+\frac{a^2}{x^4}\right)y=0.$$

This equation, when transformed, becomes

$$\left(D_z^2+\frac{2(n+1)}{z}D_z+a^2z^2\right)y=0;$$

accordingly the solution of the proposed equation is, by (3),

$$y=Ax^{2n+1}\left(\frac{d}{da}a^{-1}\right)^n\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad . \quad (7)$$

Ex. 4. Again, let

$$\left(D^2+\frac{2(n+1)}{x}D+\frac{a^2}{x^4}\right)y=0.$$

This is immediately transformed into the equation already discussed in (1); accordingly it has for its solution

$$y=A\left(\frac{d}{da}a^{-1}\right)^n\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad . \quad . \quad (8)$$

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Ex. 5. $$\left(\mathrm{D}^2-\frac{n\,.\,n+1}{x^2}+\frac{a^2}{x^4}\right)y=0.$$

This, when reduced, becomes

$$\left(\mathrm{D}_x^2+\frac{2}{x}\mathrm{D}_x-\frac{n\,.\,n+1}{x^2}+a^2\right)y=0,$$

and consequently its solution by aid of (2) is easily seen to be

$$y=\mathrm{A}x^{n+1}\left(\frac{d}{da}a^{-1}\right)^n\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad . \quad (9)$$

Ex. 6. The more general equation

$$\left(\mathrm{D}^2+\frac{2f'x}{fx}\mathrm{D}+\frac{f''x}{fx}-\frac{n\,.\,n+1}{x^2}+\frac{a^2}{x^4}\right)y=0$$

has for its solution, by (5),

$$y=\mathrm{A}\frac{x^{n+1}}{fx}\left(\frac{d}{da}a^{-1}\right)^n.\cos\left(\frac{a}{x}+\alpha\right). \quad . \quad . \quad . \quad . \quad (10)$$

In general, if we make $x=z^n$, the operation $x\frac{d}{dx}$ is equivalent to $\frac{z}{n}\frac{d}{dz}$, and accordingly $\phi(x\mathrm{D})$ is transformed into $\phi\left(\frac{z}{n}\frac{d}{dz}\right)$. This transformation leads immediately to the soluble forms of Ricati's equation; for writing it in its transformed shape, viz.

$$(\mathrm{D}^2-c^2x^{-2\lambda})y=0,$$

or

$$\left(x\mathrm{D}\,.\,(x\mathrm{D}-1)-c^2x^{2(1-\lambda)}\right)y=0;$$

if we assume $x^{1-\lambda}=z$, this equation becomes

$$\left(z\mathrm{D}_z\,.\left(z\mathrm{D}_z-\frac{1}{1-\lambda}\right)-\left(\frac{c}{1-\lambda}\right)^2z^2\right)y=0.$$

It is readily seen, that, in order that this equation should admit of a finite solution, we must have $\frac{1}{1-\lambda}=2r+1$, when r is any positive or negative whole number. Making this substitution, we get

$$\left(z\mathrm{D}\,.\,(z\mathrm{D}-2r+1)-a^2z^2\right)y=0, \text{ where } a=(2r+1)c;$$

the solution of which equation we have already seen to be (1),

$$y=\left(\frac{d}{da}a^{-1}\right)^r.\,(\mathrm{A}e^{az}+\mathrm{A}'e^{-az}),$$

or

$$=\left(\frac{d}{dc}c^{-1}\right)^r\left[\mathrm{A}\,.\,e^{(2r+1)cx^{\frac{1}{2r+1}}}+\mathrm{A}'e^{-(2r+1)cx^{\frac{1}{2r+1}}}\right], \quad (11)$$

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which contains the complete solution whenever λ is of the form $\frac{2r}{2r \pm 1}$.

For the practical application of the foregoing solutions, it is only requisite to investigate the expansion of the symbol of operation $\left(\frac{d}{da}a^{-1}\right)^n$.

It is readily seen that we may assume this expansion to be of the form

$$(Da^{-1})^n = a^{-n}D^n + A_n a^{-\overline{n+1}}D^{n-1} + B_n a^{-\overline{n+2}}D^{n-2} + \&c. + P_n a^{-2n},$$

where A_n, B_n are constants depending on n, the forms of which we have to determine.

If we operate on both sides of this equation with $\left(\frac{d}{da}a^{-1}\right)$, it becomes

$$(Da^{-1})^{n+1} = a^{-\overline{n+1}}D^{n+1} + (A_n - \overline{n+1})a^{-\overline{n+2}}D^n + \left(B_n - (n+2)A_n\right)a^{-\overline{n+3}}aD^{n-1} + \&c. \quad -(2n+1)P_n a^{-\overline{2n+1}};$$

but

$$(Da^{-1})^{n+1} = a^{-\overline{n+1}}D^{n+1} + A_{n+1}a^{-\overline{n+2}}D^n + B_{n+1}a^{-\overline{n+3}}D^{n-1} + \&c.$$

Accordingly, equating the coefficients of like powers of D, we get

$$A_n - (n+1) = A_{n+1}$$
$$B_n - (n+2)A_n = B_{n+1},$$
$$\&c.$$

hence we conclude that

$$A_n = -\frac{n.(n+1)}{2}, \quad B_n = \frac{(n-1)n\,.\,(n+1)(n+2)}{2\,.\,4},$$
$$C_n = -\frac{(n-2)\,.\,.\,.\,.\,(n+3)}{2\,.\,4\,.\,6}, \ \&c.,$$

and the complete expansion of $(Da^{-1})^n$ is

$$a^{-n}D^n - \frac{n.(n+1)}{2}a^{-\overline{n+1}}D^{n-1} + \frac{(n-1)\ldots(n+2)}{2\,.\,4}a^{-\overline{n+2}}D^{n-2} - \&c.$$
$$\pm 1\,.\,3\,\ldots\,(2n-1)a^{-\overline{2n-1}}(D-a^{-1}).$$

There is no difficulty in proving that this expansion holds for negative as well as for positive values of n; and hence by merely changing the sign of n, we might have inferred the solution of (3) from that of (1).

If we change the sign of a^2 and expand the operating symbol

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in equation (1), we immediately obtain the solution already given by Dr. Hargreave:

$$y=Ax^m e^{ax}\left(1-\frac{m\,.\,m+1}{2\,.\,ax}+\frac{(m-1)m(m+2)}{2\,.\,4\,.\,(ax)^2}-\&c.\right)$$
$$+A'x^m e^{-ax}\left(1+\frac{m\,.\,(m+1)}{2\,.\,ax}+\frac{(m-1)\,\ldots\,(m+2)}{2\,.\,4\,.\,(ax)^2}+\&c.\right).$$

Equations of the form

$$\left(fx\mathrm{D}^2+\frac{f'x}{2}\mathrm{D}+a^2\right)y=0$$

are easily reducible to the proposed form $(\mathrm{D}^2+a^2)y=0$; for if we assume $fx=\mathrm{X}^2$, then this equation becomes

$$(\mathrm{X}^2\mathrm{D}^2+\mathrm{X}\mathrm{X}'\mathrm{D}+a^2)y=0,$$

or

$$\left((\mathrm{X}\mathrm{D})^2+a^2\right)y=0; \quad \ldots\ldots\ (12)$$

hence if $z=\int\frac{dx}{\mathrm{X}}$, the equation is reduced to the required form,

$$\left(\left(\frac{d}{dz}\right)^2+a^2\right)y=0.$$

Ex. 1. $$\left((c^2-x^2)\mathrm{D}^2-x\mathrm{D}+a^2\right)y=0.$$

Here
$$z=\int\frac{dx}{\sqrt{c^2-x^2}}=\sin^{-1}\frac{x}{c};$$
$$\therefore\ y=\mathrm{A}\cos\left(a\sin^{-1}\frac{x}{c}+\alpha\right). \quad \ldots\ (13)$$

Ex. 2. $$\left((1+x^2)\mathrm{D}^2+x\mathrm{D}+a^2\right)y=0.$$

In this case
$$z=\int\frac{\mathrm{D}x}{\sqrt{1+x^2}}=\log\left(x+\sqrt{1+x^2}\right),$$

and accordingly we get

$$y=\mathrm{A}\left(x+\sqrt{1+x^2}\right)^{a\sqrt{-1}}+\mathrm{A}'\left(x+\sqrt{1+x^2}\right)^{-a\sqrt{-1}}. \quad (14)$$

Ex. 3. Again, let it be proposed to integrate

$$(\mathrm{D}^2-\cot x\mathrm{D}+a\sin^2 x)y=0.$$

In this case
$$z=\int\sin x\,dx=-\cos x,$$

and consequently we have for the solution

$$y=\mathrm{A}\cos\left(a\cos x+\alpha\right). \quad \ldots\ldots\ (15)$$

III. If we assume $x=t$, $y=tv$, then it can be readily shown

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that the operation $x\frac{d}{dx}+y\frac{d}{dy}$ is transformed into $t\frac{d}{dt}$, and accordingly the solution of all partial differential equations of the form $\phi(xD_x+yD_y)z=V$ is immediately reducible to that of the differential equation $\phi\left(t\frac{d}{dt}\right)z=V'$. More generally, if we make

$$x=t^a,$$
$$y=vt^b,$$

then the operation axD_x+byD_y is transformed into tD_t. Accordingly we can solve all partial differential equations of the form $\phi.(axD_x+byD_y)z=V$ whenever the solution of the corresponding equation $\phi\left(t\frac{d}{dt}\right)z=V'$ is known. In order to exemplify the advantage of this method of solving such classes of partial equations, I will apply it to a few examples.

Ex. 1. $$rx^2+2sxy+ty^2+a^2u_2z=0,$$

where $u_2=x^2f\frac{y}{x}$,

it is immediately seen that

$$rx^2+2sxy+ty^2=\left(x\frac{d}{dx}+y\frac{d}{dy}\right)\left(x\frac{d}{dx}+y\frac{d}{dy}-1\right)z.$$

Accordingly the proposed equation is transformed into

$$\left(t\frac{d}{dt}\cdot\left(t\frac{d}{dt}-1\right)+a^2t^2fv\right)z=0,$$

the solution of which is

$$z=\psi_1v\,.\cos at\sqrt{fv}+\psi_2v\,.\sin at\sqrt{fv},$$

or

$$z=\psi_1\frac{y}{x}\cos a\sqrt{u_2}+\psi_2\frac{y}{x}\sin a\sqrt{u_2}. \quad . \quad . \quad . \quad (16)$$

Ex. 2. $$rx^2+2sxy+ty^2+a^2u_{-2}z=0,$$

where

$$u_{-2}=x^{-2}f\left(\frac{y}{x}\right).$$

The solution of this equation evidently depends on that of $\left(\left(\frac{d}{dt}\right)^2+\frac{a_1^2fv}{t^4}\right)z=0$, and accordingly is by (6),

$$z=x\psi\frac{y}{x}\cos a\sqrt{u_{-2}}+x\psi_2\frac{y}{x}\sin a\sqrt{u_{-2}}. \quad . \quad . \quad (17)$$

Ex. 3. $$rx^2+2sxy+ty^2-2n(px+qy)+a^2u_2z=0.$$

The solution of this equation is immediately seen from (1) to be

$$z=\left(\frac{d}{da}a^{-1}\right)^n\left[\psi_1\frac{y}{x}\cos a\sqrt{u_2}+\psi_2\frac{y}{x}\sin a\sqrt{u_2}\right]. \quad (18)$$

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Ex. 4. $rx^2+2sxy+ty^2+(a^2u_2-n\,.\,n+1)z=0.$

Its solution from (2) is evidently

$$z=x^{-n}\left(\frac{d}{da}a^{-1}\right)^n\left[\psi_1\frac{y}{x}\cos a\sqrt{u_2}+\psi_2\frac{y}{x}\sin a\sqrt{u_2}\right]. \quad (19)$$

Ex. 5. $rx^2+2sxy+ty^2+(a^2u_{-2}-n\,.\,n+1)z=0.$

The solution is, by (9),

$$z=x^{n+1}\left(\frac{d}{da}a^{-1}\right)^n\left[\psi_1\frac{y}{x}\cos a\sqrt{u_{-2}}+\psi_2\frac{y}{x}\sin a\sqrt{u_{-2}}\right]. \quad (20)$$

Ex. 6.

$$rx^2+2sxy+ty^2+2(n+1)(px+qy)+a^2u_{-2}z=0.$$

The solution of this equation depends on that of (8), consequently we have

$$z=\left(\frac{d}{da}a^{-1}\right)^n\left[\psi\frac{y}{x}\,.\cos a\sqrt{u_{-2}}+\psi_2\frac{y}{x}\sin a\sqrt{u_{-2}}\right]. \quad (21)$$

Ex. 7. $rx^2+2sxy+ty^2+\left(\frac{n}{2}+1\right)(px+qy)+a^2u_{-n}z=0.$

The equation evidently depends for its solution on the differential equation

$$\left(x^{n+2}\mathrm{D}^2+\left(\frac{n}{2}+1\right)x^{n+1}\mathrm{D}+a^2\right)y=0;$$

the solution of which, by (12), is

$$y=\mathrm{A}\,.\cos\left(\frac{2a}{n}x^{-\frac{n}{2}}+\alpha\right).$$

Accordingly the proposed partial differential equation has for its solution

$$z=\psi_1\frac{y}{x}\cos\frac{2a}{n}\sqrt{u_{-n}}+\psi_2\frac{y}{x}\sin\frac{2a}{n}\sqrt{u_{-n}}. \quad . \quad . \quad (22)$$

Ex. 8. $$r+\frac{2(n+1)}{x}p-\frac{a^2}{x^4}t=0.$$

This equation is obtained from (8) by substituting $\sqrt{-1}a\frac{d}{dy}$ for a in the corresponding differential equation, consequently its solution is

$$z=\left(\frac{d}{da}a^{-1}\right)^n\left[\mathrm{F}_1\left(y+\frac{a}{x}\right)+\mathrm{F}_2\left(y-\frac{a}{x}\right)\right]. \quad . \quad (23)$$

It is unnecessary to add any further examples of this method, as the foregoing are sufficient to show its application. Most of the partial differential equations hitherto treated by the calculus of operations are readily seen to be simple cases of this method of reduction by transformation.

Trinity College, Dublin,
March 5, 1856.

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XLVI. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D.*

[Continued from p. 204.]

In the November Number of the *Annales de Chimie*, M. Baumhauer proposes a method for the determination of oxygen in organic substances. The principle on which it rests consists in burning the substance with oxide of copper, and estimating the oxygen lost by the oxide; the quantity of oxygen contained in the carbonic acid and water produced, less the quantity lost by the oxide of copper, gives the amount of oxygen contained in the substance. This determination requires special apparatus, which the author describes.

By the continued action of nitric acid on naphthaline, Laurent obtained phthalic acid, which has the formula $C^{14}H^6O^8$. Its formation from naphthaline, $C^{20}H^8$, is explained on the supposition that 4 equivs. of carbon and 2 equivs. of hydrogen are eliminated as oxalic acid. M. Dusart has found that nitronaphthaline, $C^{20}(NO^4)H^7$, undergoes a similar change when acted upon, under certain conditions, by caustic potash. In this case the nitronaphthaline loses 4 equivs. of carbon, which appear to be eliminated as such, and a body $C^{16}H^7NO^4$ is produced. This represents the nitro-compound of a hydrocarbon as yet unknown, $C^{16}H^8$, and which is isomeric with cinnamene. By acting on this new compound, which Dusart names *nitrophthaline*, with sulphide of ammonium, a base is formed which has the formula $C^{16}H^9N$. He calls it phthalidine, and describes many of its salts, as well as a substitution product obtained by acting on it with iodide of æthyle.

By a secondary action of potash on nitrophthaline a bibasic acid is produced, with the further study of which Dusart is engaged.

The same author gives a new method for the formation of the gas propylene. This consists in distilling together a mixture of an alkaline acetate and oxalate. The acetone from the destructive distillation of the acetate, coming in contact with the carbonic oxide proceeding from the decomposition of the oxalate, is deoxidized, and a gas absorbable by bromine is produced, which is propylene,—

$$\underset{\text{Acetone.}}{C^6H^6O^2} + 2CO = 2CO^2 + \underset{\text{Propylene.}}{C^6H^6}.$$

By distilling the bromide of propylene, C^6H^5Br, which the author prepared from this gas, with sulphocyanide of potassium, he obtained artificial oil of mustard.

M. Pelouze has in the same Number a short memoir on the saponification of the oils under the influence of the substances which accompany them in the seeds.

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The fatty matters contained in the seeds are neutral. When the seeds and various oleaginous kernel fruits are reduced to a state of minute division, by which the cells are destroyed, and the substances composing them put in intimate contact, the neutral fatty matters contained in them are changed into a fatty acid and glycerine. In this case a similar change is effected to that observed when the cells of the apple or grape, which isolate the ferment, are destroyed by being crushed; the sugar contained in them, acted upon by the ferment set free, is split up into alcohol and carbonic acid. M. Pelouze ascertained by direct experiments, that the fatty matters, as originally contained in the seeds, are neutral, there being only traces of fatty acids present. His mode of experimenting was to enclose seeds and grains of many different kinds in vessels which effectually excluded the air. From time to time he opened these, and determined the amount of fatty acid liberated. He found that the quantity varied directly as the time. The different kinds of neutral fatty oils varied very much in the rapidity with which they were decomposed; and this decomposition differs not only with the temperature, but with the quantities operated upon. He attempted, but without success, to isolate the ferment, by which he supposes that the decomposition is effected. In the course of his investigations he found that the sugar contained in nuts, almonds, &c., is identical with cane-sugar.

In the Australian Manna (which exudes from a species of *Eucalyptus*) Berthelot has found a crystalline saccharine matter which he names Melitose. The crystallized preparation has the formula $C^{24}H^{24}O^{24}+4HO$, and when dried at 100° C. it loses the 4 equivs. of water. Its aqueous solution deviates to the right the plane of polarization; by the addition of sulphuric acid this power is diminished by about one-third. Its behaviour with reagents is almost exactly that of cane-sugar.

But when caused to ferment, by the addition of yeast, it exhibits a striking peculiarity. While 100 parts of grape-sugar give, on fermentation, 22·2 parts of carbonic acid, the same quantity of *melitose*, which is isomeric with it, gives 44·5 parts.

When *melitose* was treated with SO^3, an uncrystallizable saccharine matter was produced. This comports itself exactly as *melitose*, and produces, like it, on fermentation, exactly half the quantity of carbonic acid which an equal weight of grape-sugar would produce.

On examining the solutions of melitose after fermentation, they were found to contain a peculiar saccharine principle which Berthelot has named *eucalyne*: its quantity was found equal to half that of the melitose employed. It deviates the plane of

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polarization to the right; it has the formula of grape-sugar, is unfermentescible, and agrees in most of its properties with sorbine.

The formation of *eucalyne* may be thus expressed:—

$$\underbrace{C^{24}H^{24}O^{24}}_{\text{Melitose.}} = 4CO^2 + 2C^4H^6O^2 + \underbrace{C^{12}H^{12}O^{12}}_{\text{Eucalyne.}}.$$

Melitose appears, then, to be formed of two isomeric compounds, of which one is fermentescible. The action of yeast disunites them, destroying the one without attacking the other.

M. Berthelot points out, from the similarity in the properties of this body to cane-sugar, the probability that this analogy extends to the constitution of cane-sugar. This, he observes, has been rendered probable by the researches of M. Dubrunfaut.

M. Berthelot has ascertained the existence, in the *Pinus Lambertianus*, of a crystallizable saccharine principle, *Pinite*, which is isomeric with quercite, and hence differs from mannite by the elements of water. From its reactions it may be ranged along with this class of substances. It forms with stearic and benzoic acids peculiar compounds, with the study of which the author is engaged.

By heating the hydrated sesquioxide of iron, $Fe^2O^3(HO)^{12}$, to 100° C. it loses one-third of the water. M. de Saint Giles finds that if this heating be prolonged some time, the change does not merely consist in a loss of water, but that the properties of the hydrate are considerably modified. Its colour is changed, it resembles the calcined oxide in appearance, and it has lost to a great extent its basic properties. It is much less easily soluble in acids, and does not produce prussian blue with ferrocyanide of potassium. It no longer exhibits the phænomena of incandescence on being heated to a dull redness, which is characteristic of the non-modified hydrate. He has also found that acetate of peroxide of iron undergoes, when heated, a similar change. Instead of water, acetic acid is liberated.

The natural ferric hydrates are divided into two classes, which correspond with these two states. Those of the first species are crystalline, and have the colour of the calcined or of the modified hydrate. They contain 10 per cent. of water, which corresponds to Fe^2O^3, HO. The second species contains the amorphous hydrates: they have a yellowish-ochreous colour, and their formula is $Fe^2O^3(HO)^{12}$, agreeing with the non-modified hydrate.

M. de Saint Giles thinks that in the modified hydrate a true allotropic transformation has been effected, and he points out that the changes which Crum has observed in the acetate of alumina are perfectly analogous. He has confirmed the observations of Crum, and has also found that the hydrate of alumina is modified by heating in a similar manner.

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M. Bineau has made some observations on the absorption of ammonia and the nitrates by cryptogamic plants. His experiments were made on the *Hydrodictyon pentagonale* and the *Conferva vulgaris*. He infers from them these consequences:—1. The demonstration of the fact of an absorption or of a decomposition of ammoniacal salts with an intensity analogous to that of CO^2. This has hitherto had no parallel in the case of saline matters, which are generally absorbed much less abundantly than their solvents. 2. That the nutrition of the Algæ is promoted by their tendency to remove the nitrates from the waters in which they vegetate, either by directly assimilating the nitrogen, or by converting the nitrates into ammoniacal salts. 3. That the elaboration by green plants of nitrogen compounds, as well as of carbonic acid, is facilitated by light.

In the November Number of Liebig's *Annalen*, Dr. Casselmann discusses at some length the process proposed by Streng for volumetric determinations by means of bichromate of potash. He communicates the results of some comparative experiments which he made with a view of testing this method, and describes the conditions under which it is applicable. He considers that the method has only a limited application for scientific purposes.

M. Toel describes the formation of cystine in the urine of two females who suffered under an inflammation of the kidneys.

The late Dr. Pauli discovered in wood-vinegar an acid which he thought to be pyrogallic acid; since his death the question has been examined by M. Buchner, who finds that it is not pyrogallic acid, but oxyphenic acid, $C^{12} H^6 O^4$. This acid invariably accompanies the products of the destructive distillation of wood, but is not found in coal-tar, being probably decomposed by the presence of ammonia.

M. Kerl proposed a method for the determination of copper, which consisted in precipitating it from its solution by means of metallic iron. The metallic copper which precipitates is then dried and weighed. M. Mohr proposes to substitute zinc for iron in this process, and gives the analytical results of determinations made in this way, which were very accurate. The copper salt or mineral is dissolved in hydrochloric acid. If nitric acid be present, it must be removed, either by lengthened boiling of the strong hydrochloric solution, or by adding a little sulphate of iron. Distilled zinc is then added. When the copper is entirely precipitated, which is ascertained by testing a drop of the solution with sulphuretted hydrogen, and the whole of the zinc dissolved, the copper is washed, dried, and weighed in a crucible.

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It was found by Fischer that nitrite of potash mixed with solutions of cobalt gave a crystalline yellowish precipitate. This body has been examined by Stromeyer, and he considers it to be composed according to the formula

$$Co^2 O^3, 2NO^3 + 3KO\,NO^3 + 2HO.$$

It is formed when a neutral cobalt salt is mixed with nitrite of potash, oxygen being absorbed, thus:—

$$2Co\,O, SO^3 + 5(KO\,NO^3) + O + 2HO = Co^2 O^3, 2NO^3 + 3(KO\,NO^3) + 2HO + 2(KO\,SO^3).$$

This reaction is of great use in detecting cobalt, as many special experiments sufficiently prove. Stromeyer gives a method for the preparation of nitrite of potash, which consists in fusing saltpetre with lead in the proportion of 1 to 2; but it seems to possess no great advantages over the ordinary methods.

Prof. Schmid of Jena communicates that he has detected urea in diabetic urine.

Dr. Vohl of Bonn gives the results of experiments made with the view of employing hyposulphite of soda as a precipitant for the heavy metallic oxides; but they do not show that this body has any decided superiority over the reagents at present in use.

In the December Number of the same journal, M. von Bibra, in a communication "On Hair and the substance of Horn," states that he has not succeeded in extracting any colouring matter from these substances; and thinks that the colours, especially of the various kinds of hair, depend on the structure, and that this is a question for the microscope. He also adduces a large number of determinations of the sulphur, fatty matter, and inorganic constituents contained in these substances. From these it is impossible to draw any useful general conclusion.

Dr. Neubauer furnishes the results of an investigation "On Catechu and its Acids," which he undertook in the hope of finding a similar connexion between catechuic acid and catechutannic acid to that which Strecker found to exist between gallic acid and tannic acid. He had hoped that by treating this acid with sulphuric acid it would split up into catechuic acid and sugar; but neither catechuic nor catechutannic acid afforded any sugar when thus treated.

He notices that the various kinds of catechu arise from the different modes of preparation, and that the catechuic acid contained in them all is of the same composition, $C^{17} H^{12} O^{10}$.

In this Number of the *Annalen* is given an abstract of the results which Piria has obtained in his investigation of Populine,

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Its formula is $C^{40}H^{22}O^{16}+4HO$, and it loses the water at 100° C. It is to be considered as composed of benzoic acid, saligenine, and grape-sugar:—

$$\underbrace{C^{40}H^{22}O^{26}+4HO}_{\text{Populine.}}=C^{14}H^{6}O^{4}+C^{14}H^{4}O^{4}+C^{12}H^{12}O^{12}.$$

Benzoic acid. Saligenine. Grape-sugar.

Salicine is composed of saligenine and grape-sugar, and when treated with acid is resolved into saliretine and grape-sugar. Similarly, populine, when treated with acid, splits up into benzoic acid, saliretine, and grape-sugar.

It was thought that if the benzoic acid could be separated from the populine, salicine would be formed. By the action of caustic baryta this is effected, and the salicine separated has also the physical and chemical properties of that prepared in the usual way. The benzoic acid formed corresponds with the quantity required by theory.

By treating salicine with nitric acid, helicine is produced. In the same way, by treating populine with nitric acid, benzo-helicine is formed. And from this benzo-helicine, by the action of caustic magnesia, helicine is produced, which is exactly the same as that produced directly from salicine.

Helicine is decomposed by acids into grape-sugar and hydride of salicyle; and benzo-helicine similarly into benzoic acid, hydride of salicyle, and grape-sugar.

Piria considers at length the conditions under which bodies form copulated compounds. When these copulated compounds split up, the elements of water are always assimilated, and in the formation of these copulated compounds water is eliminated. He proposes this law, *that when* n *constituents unite to form a copulated compound,* $2(n-1)$ *equivalents of water are eliminated.* Thus in the copulation of two bodies $=2$, of $3=4$, of $4=6$, of $5=8$. Piria considers populine an example of the copulation of three bodies, and here 4 equivs. of water are separated. Amygdaline is considered by Piria as containing five groups of atoms, in the copulation of which 8 equivs. HO are separated.

By acting on glycerine with iodide of phosphorus, Berthelot obtained a new body, the iodide of propylenyle, $C^{6}H^{5}I$. The discovery of this body has lately led to numerous important results. It occupies the same place in a new series of alcohols as iodide of æthyle does in the common alcohol series. By distilling it with sulphocyanide of potassium, Berthelot and De Luca, and independently of them, Zinin, obtained the artificial oil of mustard, $C^{2}NS^{2}K+C^{6}H^{5}I=C^{6}H^{5}C^{2}NS^{2}$. By acting with it on acetate and benzoate of silver, Zinin has obtained compounds

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which correspond to acetic and benzoic æthers. Thus,

$$C^6H^5I + AgO, C^4H^3O^3 = AgI$$
Iodide of propylenyle. Acetate of silver. Iodide of silver.

$$+ C^6H^5O, C^4H^3O^3, \text{ and } C^6H^5I$$
Acetate of propylenyle. Iodide of propylenyle.

$$AgO, C^{14}H^5O^3 = AgI + C^6H^5O, C^{14}H^5O^3.$$
Benzoate of silver. Iodide of silver. Benzoate of propylenyle.

Zinin has also obtained a carbonate of propylenyle corresponding to carbonic æther. When mercury is acted upon by iodide of propylenyle, a metallo-organic compound is formed, which corresponds to that obtained by Frankland as the result of a similar reaction with iodide of æthyle.

XLVII. *Note upon a Result of Elimination.* By A. Cayley, *Esq.**

IF the quadratic function

$$(a, b, c, f, g, h)(x, y, z)^2$$

break up into factors, then representing one of these factors by $\xi x+\eta y+\zeta z$, and taking any arbitrary quantities α, β, γ, the factor in question, and therefore the quadratic function is reduced to zero by substituting $\beta\zeta-\alpha\eta, \gamma\xi-\alpha\zeta, \alpha\eta-\beta\zeta$ in the place of x, y, z. Write

$$(a,b,c,f,g,h)(\beta\zeta-\alpha\eta, \gamma\xi-\alpha\zeta, \alpha\eta-\beta\zeta)^2=(a,b,c,f,g,h)(\alpha,\beta,\gamma)^2;$$

the coefficients of the function on the right hand are

$$\begin{aligned}
\mathrm{a}= &\quad . && c\eta^2 && +b\zeta^2 && -2f\eta\zeta && . && . \\
\mathrm{b}= && c\xi^2 && . && +a\zeta^2 && . && -2g\zeta\xi && . \\
\mathrm{c}= && b\xi^2 && +a\eta^2 && . && . && . && -2h\xi\eta \\
\\
\mathrm{f}= && -f\xi^2 && . && . && -a\eta\zeta && +h\zeta\xi && +g\xi\eta \\
\mathrm{g}= && . && -g\eta^2 && . && +h\eta\zeta && -b\zeta\xi && +f\xi\eta \\
\mathrm{h}= && . && . && -h\zeta^2 && +g\eta\zeta && +f\zeta\xi && -c\xi\eta.
\end{aligned}$$

And it is to be remarked that we have identically

$$\begin{aligned}
\mathrm{a}\xi+\mathrm{h}\eta+\mathrm{g}\zeta&=0 \\
\mathrm{h}\xi+\mathrm{b}\eta+\mathrm{f}\zeta&=0 \\
\mathrm{g}\xi+\mathrm{f}\eta+\mathrm{c}\zeta&=0.
\end{aligned}$$

Hence of the six equations, $\mathrm{a}=0$, $\mathrm{b}=0$, $\mathrm{c}=0$, $\mathrm{f}=0$, $\mathrm{g}=0$, $\mathrm{h}=0$, any three (except $\mathrm{a}=0$, $\mathrm{h}=0$, $\mathrm{g}=0$, or $\mathrm{h}=0$, $\mathrm{b}=0$, $f=0$, or $\mathrm{g}=0$, $\mathrm{f}=0$, $\mathrm{c}=0$) imply the remaining three.

* Communicated by the Author.

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If from the six equations we eliminate ξ^2, η^2, &c., we obtain

$$\square = \begin{vmatrix} . & c, & b, & -2f, & . & . \\ c, & . & a, & . & -2g, & . \\ b, & a, & . & . & . & -2h \\ -f, & . & . & -a, & h, & g \\ . & -g, & . & f, & -b, & f \\ . & . & -h, & g, & c, & -c \end{vmatrix} = 0$$

And the equation $\square = 0$ is therefore the result of the elimination of ξ, η, ζ from any three (other than the excepted combinations) of the six equations. But from what precedes, it appears that the equation $\square = 0$ must be satisfied when the quadratic function breaks up into factors, and consequently $\square$ must contain as a factor the discriminant

$$K = \begin{vmatrix} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{vmatrix}$$

of the quadratic function. This agrees perfectly with the results obtained long ago by Prof. Sylvester in his paper, "Examples of the Dialytic Method of Elimination as applied to Ternary Systems of Equations," Camb. Math. Journ. vol. ii. p. 232; but according to the assumption there made, the value of $\square$ would be (to a numerical factor *près*) *abc*K. The correct value is by actual development shown to be $\square = -2K^2$. It would be interesting to show *à priori* that $\square$ contains K^2 as a factor.

2 Stone Buildings,
March 28, 1856.

XLVIII. *On the Dynamical Theory of Heat.*—Part VI. *Thermo-electric Currents.* *By* WILLIAM THOMSON, *M.A., Professor of Natural Philosophy in the University of Glasgow.*

[Continued from p. 297.]

§§ 141–146. *Elementary Explanations in Electro-cinematics and Electro-mechanics.*

141. WHEN we confined our attention to electric currents flowing along linear conductors, it was only necessary to consider in each case the *whole strength of the current*, and the longitudinal electromotive force in any part of the circuit, without taking into account any of the transverse dimensions of the conducting channel. In what follows, it will be frequently necessary to consider distributions of currents in various directions through solid conductors, and it is therefore convenient at present to notice

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some elementary properties, and to define various terms, adapted for specifications of systems of electric currents and electromotive forces distributed in any manner whatever throughout a solid.

142. It is to be remarked, in the first place, that any portion of a solid traversed by current electricity may be divided, by tubular surfaces coinciding with lines of electric motion, into an infinite number of channels or conducting arcs, each containing an independent linear current. The *strength* of a linear current being, as before, defined to denote the quantity of electricity flowing across any section in the unit of time, we may now define the *intensity of the current* at any point of a conductor as the strength of a linear current of infinitely small transverse dimensions through this point, divided by the area of a normal section of its channel. The elementary proposition of the composition of motions, common to the cinematics of ordinary fluids and of electricity, shows that the superposition of two systems of currents in a body gives a resultant system, of which the intensity and direction at any point are represented by the diagonal of a parallelogram described upon lines representing the intensity and direction of the component systems respectively. Hence we may define the components, along three lines at right angles to one another, of the intensity of electric current through any point of a body, as the products of the intensity of the current at that point into the cosines of the inclination of its direction to those three lines respectively; and we may regard the specification of a distribution of currents through a body as complete, when the components parallel to three fixed rectangular axes of reference of the intensity of the current at every point are given.

143. The term electromotive force has been applied in what precedes, consistently with the ordinary usage, to the whole force urging electricity through a linear conducting arc. When a current is sustained through a conducting arc by energy proceeding from sources belonging entirely to the remainder of the circuit, the electromotive force may be considered as applied from without to its extremities; and in all such cases it may be measured —electrostatically, by determining in any way the difference of potential between two conducting bodies insulated from one another and put in metallic communication with the extremities of the conducting arc;—or electro-dynamically, by applying to these points the extremities of another linear conductor of infinitely greater resistance (practically, for instance, a long fine wire used as a galvanometer coil), and determining the strength of the current which branches into it when it is so applied. These tests may of course be regarded as giving either the amount of the electromotive force with which the remainder of the circuit acts on, or the whole of the electromotive force efficient in, the

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passive conducting are first considered. On the other hand, the electromotive force acting in the portion from which the energy proceeds is not itself determined by such tests, but is equal to the whole electromotive force of the sources contained in it, diminished by the reaction of the force which is measured in the manner just explained. The same tests applied to any two points whatever of a complete conducting circuit, however the sources of energy are distributed through it, show simply the electromotive force acting and reacting between the two parts into which the circuit might be separated by breaking it at these points. In some cases, for instance some cases of thermo-electric action which we shall have to consider*, these tests would give a zero indication to whatever two points of a circuit through which a current is actually passing they are applied, and would therefore show that there is no electric action and reaction between different parts of the circuit, but that each part contains intrinsically the electromotive force required to sustain the current through it at the existing rate. An actual test of the electromotive force of sources contained in any part of a linear conductor is defined, with especial reference to the circumstances of thermo-electricity, in the following statement:—

144. *Def.* The actual intrinsic electromotive force of any part of a linear conducting circuit is the difference of potential which it produces in two insulated conductors of a standard metal at one temperature, when its extremities are connected with them by conducting arcs of the same metal, and insulated from the remainder of the circuit.

The electromotive force so defined may be determined, either by determining by some electrostatical method the difference of potentials in the two conductors of standard metal mentioned in the definition, or by measuring the strength of the current produced in a conducting arc of the standard metal of infinitely greater resistance than the given conducting arc, applied to connect its extremities when insulated from the remainder of its own circuit.

145. With reference to the distribution of electromotive force through a solid, the following definitions are laid down:—

Def. 1. The intrinsic electromotive force of a linear conductor at any point is the actual intrinsic electromotive force in an infinitely small arc through this point divided by its length.

Def. 2. The efficient electromotive force at any point of a linear conducting circuit is the sum of the actual intrinsic electromotive force in an infinitely small arc, and the electromotive force produced by the remainder of the circuit on its extremities, divided by its length.

* For one of these see § 167 below.

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Def. 3. The intrinsic electromotive force in any direction, at any point in a solid, is the electromotive force that would be experienced by an infinitely thin conducting arc of standard metal, applied with its extremities to two points in a line with this direction, in an infinitely small portion insulated all round from the rest of the solid, divided by the distance between these points.

Def. 4. The electromotive force efficient at any point of a solid, in any direction, is the difference of the electromotive forces that would be experienced by an infinitely thin conducting arc of standard metal, with its extremities applied to two points infinitely near one another in this direction, divided by the distance between the points, in the two cases separately of the solid being left unchanged, and of an infinitely small portion of it containing these points being insulated from the remainder.

146. Principle of the superposition of thermo-electric action. It may be assumed as an axiom, that each of any number of co-existing systems of electric currents produces the same reversible thermal effect in any locality as if it existed alone.

§§ 147–155. *On Thermo-electric Currents in Linear Conductors of Crystalline Substance.*

147. The general characteristic of crystalline matter is, that physical agencies, having particular directions in the space through which they act, and depending on particular qualities of the substance occupying that space, take place with different intensities in different directions if the substance be crystalline. Substances not naturally crystalline may have the crystalline characteristic induced in them by the action of some directional agency, such as mechanical strain or magnetization; and may be said to be inductively crystalline. Or again, minute fragments of non-crystalline substances may be put together so as to constitute solids, which on a large scale possess the general characteristic of homogeneous crystalline substances; and such bodies may be said to possess the crystalline characteristic by structure, or to be structurally crystalline.

148. As regards thermo-electric currents, the characteristic of crystalline substance must be, that bars cut from it in different directions would, when treated thermo-electrically as linear conductors, be found in different positions in the thermo-electric series; or that two bars cut from different directions in the substance would be thermo-electrically related to one another like different metals. This property has been experimentally demonstrated by Svanberg for crystals of bismuth and antimony; and there can be no doubt but that other natural metallic crystals will be found to possess it. I have myself observed, that the

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thermo-electric properties of copper and iron wires are affected by alternate tension and relaxation in such a manner as to leave no doubt but that a mass of either metal, when compressed or extended in one direction, possesses different thermo-electric relations in different directions. Fragments of different metals may be put together so as to form solids, possessing by structure the thermo-electric characteristic of a crystal, in an infinite variety of ways. Thus, a structure consisting of thin layers alternately of two different metals, possesses obviously the thermo-electric qualities of a crystal with an axis of symmetry. I have investigated the thermo-electric properties in all directions of such a structure in terms of the conducting powers for heat and electricity, and the thermo-electric powers, of the two metals of which it is composed; and bars made up of alternate layers of copper and iron, one with the layers perpendicular, another with the layers oblique, and a third with the layers parallel to the length, illustrating the theoretical results which were communicated along with this paper, were exhibited to the Royal Society. The principal advantage of considering metallic structures with reference to the theory of thermo-electricity is, as will be seen below, that we are so enabled to demonstrate the possibility of crystalline thermo-electric qualities of the most general conceivable type, and are shown how to construct solids (whether or not natural crystals may be ever found) actually possessing them.

149. The following two propositions with reference to thermo-electric effects in a particular case of crystalline matter are premised to the unrestricted treatment of the subject, because they will serve to guide us as to the nature of the agencies for which the general mathematical expressions are to be investigated.

Prop. I. If a bar of crystalline substance, possessing an axis of thermo-electric symmetry, has its length oblique to this axis, a current of electricity sustained in it longitudinally will cause evolution of heat at one side and absorption of heat at the opposite side, all along the bar, when the whole substance is kept at one temperature.

Prop. II. If the two sides of such a bar be kept at different temperatures, and a homogeneous conducting arc be applied to points of the ends which are at the same temperature, a current will be produced along the bar, and through the arc completing the circuit.

150. For proving these propositions, it will be convenient to investigate fully the thermo-electric agency experienced by a bar cut obliquely from a crystalline substance possessing an axis of symmetry, when placed longitudinally in a circuit of which the remainder is composed of the standard metal, and kept with either its sides or its ends unequally heated. Let θ and ϕ

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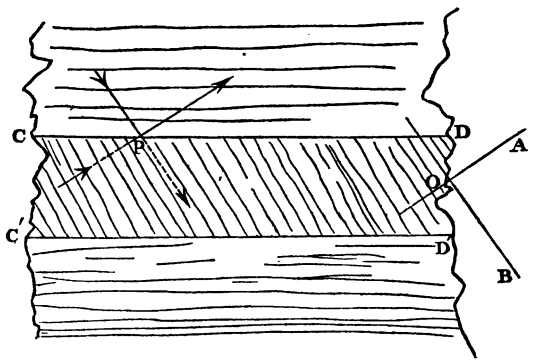
denote the thermo-electric powers of two bars cut from the given substance in directions parallel and perpendicular to its axis of symmetry respectively. Let us suppose the actual bar to be of rectangular section, with two of its opposite sides perpendicular to the plane of its length and the axis of symmetry of its substance. Let a longitudinal section in this plane be represented by the accompanying diagram; let OA or any line parallel to it be the direction of the axis of symmetry through any point; and let ω denote the inclination of this line to the length of the bar. Let the breadth of the two opposite sides of the bar perpendicular to the plane of the diagram be denoted by a, and in the plane of the diagram b. The area of the transverse section of the bar will be ab; and therefore if γ denote the strength, and i the intensity, of the current in it, we have (§ 142)

$$i = \frac{\gamma}{ab}.$$

151. We may suppose the current, itself parallel to the length of the bar, and in the direction from left to right of the diagram, to be resolved (§ 142) at any point P at the side of the bar into two components in directions parallel and perpendicular to OA, of



which the intensities will be $i \cos \omega$ and $i \sin \omega$ respectively. The former of these components may be supposed to belong to a system of currents crossing the bar in lines parallel to OA, and passing out of it across the side CD into a conductor of the standard metal; and the latter, to a system of currents entering the bar across CD from the same conductor of standard metal, and crossing it in lines perpendicular to OA. The resultant current in the supposed standard metal beside the bar will clearly be parallel to the length, and can therefore (this metal being non-crystalline) produce no effect influencing the thermal agency at the side of the bar or within it. The inclinations of the currents to a perpendicular to the separating plane of the two metals

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being respectively $90°-\omega$ and ω, their strengths per unit of area of this plane, obtained by multiplying their intensities by the cosine of those angles respectively, will be each equal to

$$i \cos \omega \sin \omega.$$

Hence the absorptions of heat which they will produce at the surface of separation of the metals per unit of area per second will be

$$-\frac{1}{J} i \cos \omega \sin \omega t\theta, \text{ and } \frac{1}{J} i \cos \omega \sin \omega t\phi,$$

respectively. According to the general principle of the superposition of thermo-electric actions stated above, the sum of these is the rate of absorption of heat per unit of surface when the two systems of currents coexist. But the resultant of these systems is simply the given longitudinal current in the bar, with no flow either out of it or into it across any of its sides. Hence a simple current of intensity i, parallel to the sides of the bar, causes absorption of heat at the side CD amounting to

$$\frac{1}{J} i \cos \omega \sin \omega t(\phi - \theta),$$

per unit of area per second; and the same demonstration shows that an equal amount of evolution must be produced at the opposite side C′D′. These effects take place quite independently of the matter round the bar, since the metal carrying electric currents which we supposed to exist at the sides of the bar in the course of the demonstration, can exercise no influence on the phænomena.

152. If l denotes the length of the bar, the area of each of the sides perpendicular to the plane of the diagram will be la; and therefore the absorption over the whole of the side CD, and the evolution over the whole of the other side C′D′, per second, will be

$$\frac{1}{J} ila \cos \omega \sin \omega t(\phi - \theta),$$

or

$$\frac{1}{J} \gamma \frac{l}{b} \cos \omega \sin \omega t(\phi - \theta).$$

It is obvious that there can be neither evolution nor absorption of heat at the two other sides.

153. An investigation similar to that which has just been completed, shows that if the actual current enter from a conductor of the standard metal at one end of the bar, and leave it by a conductor of the same metal at its other end, the absorption and evolution of heat at these ends respectively will amount to

$$\frac{1}{J} \gamma (t\theta \cos^2 \omega + t\phi \sin^2 \omega)$$

per second.

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154. Let us now suppose the two sides CD, C'D' to be kept at uniform temperatures, T, T', and the two ends to be kept with equal and similar distributions of temperatures, whether a current is crossing them or not. Then if a current of strength γ be sent through the bar from left to right of the diagram, in a circuit of which the remainder is the standard metal, there will be reversible thermal action, consisting of the following parts, each stated per unit of time.

(1.) Absorption amounting to $\Omega(T)\frac{l}{b}\gamma$, in a locality at the temperature T.

(2.) Evolution amounting to $\Omega(T')\frac{l}{b}\gamma$, in a locality at the temperature T'.

(3.) Absorption amounting to $\Pi\gamma$ at one end (that beyond CC'),

and (4.) Evolution amounting to $\Pi\gamma$ at the other end;

where, for brevity, $\Omega(T)$ and $\Omega(T')$ are assumed to denote the values of $\frac{t}{J}(\phi-\theta)\sin\omega\cos\omega$ at the temperatures T and T'; and Π the mean value of $\frac{t}{J}(\theta\cos^2\omega+\phi\sin^2\omega)$ for either end of the bar. The contributions towards the sums appearing in the general thermo-dynamic equations which are due to these items of thermal agency are as follows:—

$$\left[\Omega(T)-\Omega(T')\right]\frac{l}{b}\gamma \qquad \text{towards } \Sigma H_t,$$

and

$$\left[\frac{\Omega(T)}{T}-\frac{\Omega(T')}{T'}\right]\frac{l}{b}\gamma \qquad \text{towards } \Sigma\frac{H_t}{t};$$

the thermal agencies at the ends disappearing from each sum in consequence of their being mutually equal and opposite, and similarly distributed through localities equally heated. Now when every reversible thermal effect is included, the value of $\Sigma\frac{H_t}{t}$ must be zero, according to the second general law. Hence either $\frac{\Omega(T)}{T}-\frac{\Omega(T')}{T'}$ must vanish, or there must be a reversible thermal agency not yet taken into account. But probably $\frac{\Omega(T)}{T}-\frac{\Omega(T')}{T'}$ may not vanish, that is $\frac{\Omega}{t}$ may vary with the temperature, for natural crystals; and it certainly does vary with the temperature for metallic combinations structurally crystalline (for instance, for a bar cut obliquely from a solid consisting of

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alternate layers of copper and iron, the value of Ω decreases to zero as the temperature is raised from an ordinary atmospheric temperature up to about 280°, and has a contrary sign for higher temperatures). Hence in general there must be another reversible thermal agency, besides the agencies at the ends and at the sides of the bar which we have investigated. This agency must be in the interior; and since the substance is homogeneous, and uniformly affected by the current, the new agency must be uniformly distributed through the length, as different points of the same cross section can only differ in virtue of their different circumstances as to temperature. If there were no variation of temperature, there could be no such effect anywhere in the interior of the bar; and therefore if dt denote the variation of temperature in an infinitely small space dx across the bar in the plane of the diagram, and χ an unknown element, constant or a function of the temperature, depending on the nature of the substance, we may assume

$$i\chi\frac{dt}{dx}$$

as the amount of absorption, per unit of the volume of the bar, due to a current of intensity i, by means of the new agency. The whole amount in a lamina of thickness dx, length l, and breadth a perpendicular to the plane of the diagram, is therefore

$$i\chi\frac{dt}{dx}aldx,$$

or

$$\gamma\frac{l}{b}\chi dt.$$

As there cannot possibly be any other reversible thermal agency to be taken into account, we may now assume

$$\Sigma H_t=\gamma\frac{l}{b}\left\{[\Omega(T)-\Omega(T')]+\int_{T'}^{T}\chi dt\right\} \quad . \quad (22),$$

$$\Sigma\frac{H_t}{t}=\gamma\frac{l}{b}\left\{\frac{\Omega(T)}{T}-\frac{\Omega(T')}{T'}+\int_{T'}^{T}\frac{\chi}{t}dt\right\} \quad . \quad (23).$$

The second general law, showing that $\Sigma\frac{H_t}{t}$ must vanish, gives by the second of these equations,

$$\frac{\Omega(T)}{T}-\frac{\Omega(T')}{T'}+\int_{T'}^{T}\frac{\chi}{t}dt=0 \quad . \quad . \quad . \quad (24).$$

Substituting in place of T, t, and differentiating with reference to this variable, we have as an equivalent equation,

$$\frac{\chi}{t}=-\frac{d\frac{\Omega}{t}}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25);$$

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and using this in (22), we have

$$\Sigma H_t = \gamma \frac{l}{b} \int_{T'}^{T} \frac{\Omega}{t} dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26).$$

This expresses the full amount of heat taken in through the agency of the current γ, of which the mechanical equivalent is therefore the work done by the current. Hence (according to principles fully explained above, §§ 109, 110) the thermal circumstances must actually cause an electromotive force F, of which the amount is given by the equation

$$F = J \frac{l}{b} \int_{T'}^{T} \frac{\Omega}{t} dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27),$$

to act along the bar from left to right of the diagram; which will produce a current unless balanced by an equal and contrary reaction. This result both establishes Proposition II., enunciated above in § 149, and shows the amount of the electromotive force producing the stated effect in terms of T and T', the temperatures of the two sides of the bar, the obliquity of the bar to the crystalline axis of symmetry, and the thermo-electric properties of the substance; since if θ and ϕ denote its thermo-electric powers along the axis of symmetry, and along lines perpendicular to this axis, at the temperature t, and ω the inclination of this axis to the length of the bar when the substance is at the temperature t, we have

$$\Omega = \frac{t}{J} (\phi - \theta) \sin \omega \cos \omega \quad . \quad . \quad . \quad . \quad . \quad (28).$$

155. By an investigation exactly similar to that of § 115, which had reference to non-crystalline linear conductors, we deduce the following expression for the electromotive force, when the ends of the bar are kept at temperatures T, T' from the terminal thermal agency Π, of a current investigated in § 153:—

$$F = J \int_{T'}^{T} \frac{\Pi}{t} dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29),$$

where

$$\Pi = \frac{t}{J} (\theta \cos^2 \omega + \phi \sin^2 \omega) \quad . \quad . \quad . \quad . \quad (30).$$

[To be continued.]

XLIX. *On the Discovery of the true form of* Carnot's *Function.* *By* R. Clausius.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper communicated to the Royal Society of Edinburgh in 1851*, Prof. W. Thomson ascribed to Mr. J. P. Joule the discovery of the theorem, that Carnot's function, which Cla-

* Edinb. Trans. vol. xx.; and Phil. Mag. 4th series, vol. ix.

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peyron expressed by C, and Thomson by the fraction $\frac{1}{\mu}$, "is nothing more than the absolute temperature multiplied by the equivalent of heat for the unit of work." I have hitherto avoided mentioning this point in my papers; principally because I have so high an esteem for the labours of the physicist for whom Prof. Thomson claims priority, that I was anxious to avoid even the appearance of wishing to lessen his deserts. But as Prof. Thomson has since then frequently repeated that assertion,—among other places in the paper in the March Number of this Journal, where, in page 215, he calls that theorem "Mr. Joule's conjecture,"—I think it necessary to say a few words on the subject.

Holtzmann established the same formula for the function C in a paper which appeared as early as 1845*; and Helmholtz, in his pamphlet published in 1847, "On the Conservation of Force," citing Holtzmann's paper, calculated several values obtained by this formula, and compared them with those arrived at by Clapeyron in a different manner. But the views upon which Holtzmann founded his speculations do not agree with the mechanical theory of heat as at present received; so that after this had been recognized, the correctness of the formula found by him was, naturally, again rendered doubtful. On this account, in a paper communicated to the Berlin Academy in February 1850†, "On the Moving Force of Heat," in which I brought Carnot's proposition in agreement with the mechanical theory of heat, I again endeavoured to determine his function more accurately. Therein I arrived at the same formula as Holtzmann, and I believe that I then, for the first time, correctly explained the principles upon which this formula is based.

In presence of these facts, Prof. Thomson, to justify his statement, says‡, "It was suggested to me by Mr. Joule, in a letter dated December 9, 1848, that the true value of μ might be inversely as the temperature from zero." Against this I must beg to urge,—*First*, that, as far as I am aware, it is usual, in determining questions of priority in scientific matters, only to admit such statements as have been *published*. And I believe that this custom ought to be conscientiously adhered to, especially in theoretical investigations; for it usually requires continued and laborious research in order to give to a thought, after it has been first entertained, and perhaps casually communicated to a friend, that degree of certainty which is necessary before venturing upon its publication. *Secondly*, that since Thomson does not say that

* On the Heat and Elasticity of Gases and Vapours. By C. Holtzmann. Mannheim, 1845.

† Poggendorff's *Annalen*, vol. lxxix.; and Phil. Mag. 4th series, vol. ii.

‡ Edinburgh Transactions, vol. xx. p. 279.

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Mr. Joule had proved the theorem, but only that he had offered it as an opinion, I do not see why this opinion should have the priority over that which Holtzmann had arrived at three years before.

In conclusion allow me to make one remark. In a more recent paper, "On a Modified Form of the Second principal Theorem in the Mechanical Theory of Heat*," I have introduced, instead of Carnot's function C, another function of the temperature, which I have designated by T, and by which all developments are very much simplified. This function has a determinate relation to that of Carnot's, which I have expressed by the equation

$$\frac{\frac{dT}{dt}}{T} = \frac{A}{C},$$

in which t represents the temperature, and A the equivalent of heat for unit of work. It is easily recognized, that, according to this equation, the functions C and T are in general to be considered different; but that for the special case, in which C is proportional to the absolute temperature, T must be also proportional to it. And in fact I have shown, from the same principles which before led me to the determination of C, that in all probability T is simply the absolute temperature itself.

I remain, Gentlemen,

With great respect, yours &c.,

Zurich, March 20, 1856. R. Clausius.

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 306.]

December 6, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

THE following communication was read :—

"On Chemical Affinity, and the Solubility of the Sulphate of Baryta in Acid Liquors." By F. Crace Calvert, Esq.

Solubility of the Sulphate of Baryta.

The author observes that sulphate of baryta is not an insoluble salt, as is generally admitted, for he has found that 1000 grs. of nitric acid, of spec. grav. 1·167, are capable of dissolving 2 grs. of sulphate of baryta; and what renders the knowledge of this fact still more useful in analytical chemistry is, that the insolubility of this salt is affected even by the weakest nitric or hydrochloric acids; for whilst 0·062 gr. of sulphate of baryta only requires 1000 grs. of nitric acid, of spec. grav. 1·032, to hold it in solution, the same quantity of salt requires 50·000 grs. of pure distilled water to dissolve it.

* Poggendorff's *Annalen*, vol. xciii.

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What is not less useful to know is, that the solubility of sulphate of baryta is affected in a higher degree by the bulk of the acid than by its strength. The two following tables, taken from amongst many others contained in the paper, will not only illustrate hi s fact, but will also give an insight into the way in which the experiments were conducted. The first table illustrates the influence which increasing bulks of the same nitric acid exert on the formation of sulphate of baryta, and the second table the action which increasing strengths of acid have:—

TABLE XVI.

Order of jars.	Number of divisions of the alkalimeter of nitric acid, spec. grav. 1·167.	Number of divisions of the alkalimeter of water added.	Spec. grav. of the bulk of acid.	Sulphate of potash dissolved in the acid.	Nitrate of baryta poured in, previously dissolved in 20 grs. of water.	Time required for a precipitate to appear.	Quantity of sulphate of baryta precipitated.	Quantity of sulphate of baryta dissolved.
1	20	20	1·167	3·34	5·00	3 min.	4·28	Average quantity dissolved equal 0·10 gr.
2	20	40	1·120	...	...	...	4·34	
3	20	60	1·085	...	...	...		
4	20	80	1·067	...	...	...		
5	20	100	1·057	...	...	...	4·35	
6	20	120	1·050	...	...	...	4·35	
7	20	140	1·044	...	...	...	4·36	
8	20	160	1·039	...	...	...		
9	20	180	1·035	...	...	...		
10	20	200	1·032	...	...	...	4·38	

TABLE II.

Order of jars.	Number of divisions of the alkalimeter.	Corresponding weight of nitric acid, sp. gr. 1·167.	Quantity of sulphate of potash.	Quantity of nitrate of baryta.	Weight of sulphate of baryta.	Time required for a precipitate to appear.	Quantity of sulphate of baryta dissolved.
1	40	466·8	3·34	5·00	4·46	Instantly	0·02
2	80	933·6	...	...	...	20 minutes	1·29
3	120	1400·4	...	...	...	2 hours	2·34
4	160	1867·2	...	...	...	8½ hours	3·66
5	200	2334·0	...	...	...	24 hours	
6	240	2800·8	...	...	...	No precip.	
7	280	3267·6	...	...	...		
8	320	3734·4	...	...	...		
9	360	4201·2	...	...	...		
10	400	4668·0	...	...	...		

These tables clearly show the influence which a given strength of nitric acid has on the solubility of the sulphate of baryta; for there is a precipitate in three minutes in all the jars of the first table, whilst we have a precipitate only in the first four jars of Table II.

Another fact which is observed in these tables is, that whilst 240 divisions of the alkalimeter of nitric acid, spec. grav. 1·167, are capable of dissolving, or preventing the formation of, 4·46 grs. of sulphate of baryta, 240 divisions of an acid, of spec. grav. 1·032, only retained in solution 0·086 gr. It follows from these facts, that

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in future the practice of rendering liquors acid with nitric or hydrochloric acids, must be discontinued when sulphates are to be determined, or separated from chromates, phosphates, &c.

Influence of Mass on Chemical Affinity.

The researches of the author, to illustrate the influence which mass exerts on chemical affinity, are extensive; a few of the results arrived at are here given.

The following table will clearly show the marked influence which increasing volumes of nitric acid have in preventing the formation of sulphate of baryta :—

TABLE IV.

Number of jars.	Number of divisions of the alkalimeter.	Corresponding weight of acid, sp. gr. 1·167.	Quantity of sulphate of potash.	Quantity of nitrate of baryta.	Weight of sulphate of baryta.	Time when precipitate appeared.
1	40	466·8	5·12 grs.	8·00	7·13	Instantly
2	80	933·6	...	...	...	2 minutes
3	120	1400·4	...	...	...	14 minutes
4	160	1867·2	...	...	...	1 hour
5	200	2334·0	...	...	...	1 hour 15 minutes
6	240	2800·8	...	...	...	4 hours
7	280	3267·6	...	...	...	8 hours [cloud)
8	320	3734·4	...	...	...	21 hours (only a
9	360	4201·2	...	...	...	None
10	400	4668·0	...	...	...	None

Thus in this table we perceive, that as the bulk of acid increases, more time is required for a precipitate to appear, although there is a large excess of substance employed on the quantity necessary to give an instantaneous precipitate; and it is curious to observe how wide is the space of time in each successive jar for a precipitate to appear, and in jars numbers 9 and 10 no deposits were formed after twenty-four hours. As the quantity of precipitate decreased rapidly in each successive jar, they were gathered, and their amount determined with due care; and these are the facts observed :—

TABLE V.

Number of jars.	Number of divisions of the alkalimeter.	Corresponding weight of nitric acid, sp. gr. 1·167.	Quantity of sulphate of baryta precipitated.	Quantity of sulphate of baryta dissolved.	Quantity of sulphate of baryta dissolved per 1000 grs.
1	40	466·8	6·86	0·27	0·591
2	80	933·6	5·63	1·50	1·615
3	120	1400·4	4·66	2·47	1·767
4	160	1867·2	3·22	3·91	2·099
5	200	2334·0	2·33	4·80	2·059
6	240	2800·8	1·10	6·03	2·155
7	280	3267·6	0·14	6·99	2·141

The results contained in this table, especially those in the last column, clearly show the influence of mass on chemical affinity, for there is no difference in any of the jars excepting the increasing bulk

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of the acid; and still we have in jar No. 1 only 0·591 of sulphate of baryta dissolved per 1000 grs. of acid, whilst we have 2·099 in No. 4 jar.

But the relative bulk of acid is not the only influence which affects the affinity of sulphuric acid for baryta, as the relative quantities of nitrate of baryta and sulphate of potash put in presence also exert an action. This fact is illustrated by the following results, taken from three different tables, in which the same quantities of acid were used, but different proportions of salts:—

TABLE IV. E.

Number of table.	Quantities of acid, sp. gr. 1·167.	Sulphate of potash.	Nitrate of baryta.	Sulphate of baryta.	Time before a precipitate appeared.
No. 1	466·8	0·753	1·121	1·00	12 hours
	933·6	0·753	1·121	1·00	None
No. 2	466·8	3·34	5·00	4·46	Instantly
	933·6	3·34	5·00	4·46	2 hours
No. 4	466·8	5·12	8·00	7·13	Instantly
	933·6	5·12	8·00	7·13	2 minutes

This point is again brought out in the following table, which is also taken from several series of experiments:—

TABLE IV. A.

Number of table.	Order of jar.	Quantities of fluid.	Total quantities of sulphate of baryta susceptible of being produced.	Quantity of acid represented.	Quantities of sulphate of baryta dissolved in 1000 grs.	Increased ratio of solubility.
No. 1	2	933·6	1·00	1000	1·071	0·
No. 2	6	2800·8	4·46	...	1·593	0·522
No. 4	9	4201·2	7·13	...	1·912	0·841

These facts, and others described in the paper, demonstrate that the solubility of the sulphate of baryta, or its non-formation, is not only influenced by the respective bulks of an acid of spec. grav. 1·167, and the respective quality of salts employed, but that the relative quantity of matter put in presence has a decided influence on chemical affinity; and these observations not only corroborate perfectly the results obtained by Mr. Bunsen on the influence of volumes on the combination of gases, and the observations which show a like influence on the carbonates, but also are, I believe, the first instance which has been noticed of irregularity of solubility of a substance in increased multiple bulks of a liquid.

GEOLOGICAL SOCIETY.

[Continued from p. 315.]

March 19, 1856.—D. Sharpe, Esq., President, in the Chair.

The following communications were read:—

1. "On some Organic Remains from the Bone Bed at the base of the Lias at Lyme Regis." By the Rev. Mr. Dennis. Communicated by Sir C. Lyell, V.P.G.S.

In this communication the author drew attention to some peculiar

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bones and teeth from the Bone-bed which occurs between the Trias and the Lias. Mr. Dennis considered that some of these fossils presented mammalian structure under the microscope. Among the specimens from the Lyme Regis bone-bed, Prof. Owen determined the remains of *Lepidotus* and *Saurichthys*, and of another fish, *Placodus*, which had not previously been recognized among British fossils.

2. "On the Valenciennes Coal-basin." By M. Laurent. In a Letter to A. Tylor, Esq., F.G.S.

This communication referred to the works in the coal-basin of the departments of the "Nord" and the "Pas de Calais," on the prolongation of the Belgian basin of Mons. At the end of the last century, France in the north possessed only the mines of Auzin, which were first worked in 1716. This state of things lasted until 1832, when the workings only extended to Denain. In 1839, the concessions of Douchy, Bruelle, Vicoigne, Auiche, Agincourt, and Thioncelles were made. The works of research went on until 1841, at which period the adventurers, discouraged by the numerous fruitless attempts made in the supposed direction of the basin towards Arras, abandoned them. Six years later, the works undertaken towards the north-west of Douai, in the direction of the present concessions of the "Pas de Calais," indicated the true direction of the coal-basin; and down to 1854, numerous trial-sinkings, of which many passed through the coal, led to the establishment of nine new concessions; and a tenth, on the border of the basin, is in progress. Two more also have been made this year (1855), one to the north of Douai, the other to the north of Bethune, above Choques, where they suppose that the bands of dry coal (*faisceau maigre*) end, the basin beyond this place becoming narrower, and representing only the seams of caking coal in all the concessions to the west.

Many works have, moreover, been undertaken in the course of the last three years, in search of a widening of the basin by the series of the seams of caking coal, and of an extension of the dry-coal band, which disappears at Choques. With the exception of those made by the "Vendin Company," these sinkings have as yet given only negative results.

In all the sinkings which have been made from Valenciennes to the furthest of these researches, the Chalk forms the (*mort terrain*) "head," and with a varying thickness. As far as Aire the Chalk alone forms the rock which has to be passed through before reaching the Coal, from which it is separated by a bed of greensand from 1 to 3 metres in thickness, known by the name of "tourtia." On the north of Aire it is, in addition, covered up by tertiary deposits, alternations of sands and clays, with a thickness in places of 100 to 150 metres, and which render it necessary to line the sinkings as the work advances. This formation is found even in Belgium, at St. Ghislain, near Mons, with a thickness of 60 metres. The average thickness of the overlying beds is 140 metres. It seldom exceeds 180 metres, and was found to be only 85 metres at

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Marles, near Bethune. It is near this town that the depth to the base of the Chalk is the greatest; the sinkings which have been conducted on the south gave a result at a smaller depth.

Nearly 2,000,000 francs have been expended by various companies, all formed of private persons; and in more than 150 sinkings numerous workings have resulted, which have increased beyond all expression the wealth of these two departments, have developed a portion of the coal-field of France, and enriched on a grand scale the fortunate adventurers. The small basin of Fiennes and Hardinghem, near Guisnes, is independent of this large one; it is a coal-deposit in the Mountain Limestone, and which has been worked for some time past for local consumption; the coal is found at a slight depth, but the quantity of water renders the workings both difficult and expensive.

Similar works are being carried on in the department of the Moselle, where they are tracing the prolongation of the Sarrebruck basin beneath the New Red Sandstone. Eight companies have already met with the coal between 200 and 300 metres in depth, and are applying for concessions. It is in this quarter and in the department Nord that the principal search is now being made.

This letter was accompanied by an outline map of the district referred to, on which were shown the extent of the several concessions, the position and character of the most important of the borings, and approximatively the length and breadth of the Valenciennes coal-basin as indicated by the workings hitherto effected.

3. "On the Sandstones and Breccias of the South of Scotland of an age subsequent to the Carboniferous Formation." By Prof. Harkness, F.G.S.

The author first referred to a former paper, in which he had described in detail some of these sandstones and breccias, especially those of the neighbourhood of Dumfries. He then gave his reasons for regarding the sandstones of Annan in the south-east of Dumfriesshire to be continuous with, and of the same age as, those of Carlisle, viz. of the Triassic age; and pointed out several patches of sandstone and breccia in other parts of Dumfriesshire and in Ayrshire, which lie either on the Carboniferous or the Lower Silurian rocks of the district, and are probably referable to the Permian epoch.

These sandstones and breccias appear to have been deposited subsequently to the eruption of the trap-dykes that have dislocated the coal-fields of central Scotland, and to have been always derived from the neighbouring older rocks. The author divides them into four distinct groups, viz. 1st (the lowest), breccias and sandstones, best seen in the course of the Kinnel Water and at Ballochmoyle in Ayrshire; 2ndly, sandstones, for the most part false-bedded, well seen in the Corncockle area, the Thornhill district, at Mauchline, and in the vicinity of Dumfries; 3rdly, hard thick breccias, best seen at the Craigs, Dumfries; and 4thly, thin-bedded sandstone, only slightly developed, occurring at Castledikes, Dumfries, above the breccia.

Animal life abounded, in the form of Reptiles, during the period

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of the deposition of these Permian beds, as evidenced by the numerous impressions of foot-tracks of Chelonians, Lizards, and Batrachians, which walked over the shores of the Permian waters, when the sandstones of Corncockle Muir and Dumfries were sandy beaches with mud-patches scattered over them.

Mr. Harkness regards the several patches or areas of the rocks in question as having once been connected in a mass of great superficial extent; and he thinks it probable that the denudation which is supposed to have removed the greater portion took place in the pleistocene epoch; the preservation of the isolated patches being due to local subsidences.

April 9, 1856.—D. Sharpe, Esq., President, in the Chair.

The following communications were read:—

1. "Notes on the Geology of the neighbourhood of Sydney and of Brisbane, Australia." By J. S. Wilson, Esq. Communicated by Sir R. I. Murchison, F.G.S.

Mr. Wilson, the Geologist of the North Australian Expedition, availing himself of such opportunities as occurred to make geological notes on the tracts hastily visited on his way to Cambridge Gulf, where the party had disembarked, communicated a series of observations on the sandy Carboniferous Rocks of Sydney, the coal of the Hunter River, the structure of Nobby's Island in Newcastle Harbour, the geological characters of the district from Newcastle, through Maitland and Singleton, to the Liverpool Range, the gold-diggings at the Hanging Rocks, and the Peel River district. After returning to Sydney, Mr. Wilson sailed to Moreton Bay, where he again had an opportunity of studying the Carboniferous strata, as well as the crystalline rocks around Brisbane; the latter are to some extent auriferous.

2. "On the Strata of Hastings Cliffs." By S. H. Beckles, Esq., F.G.S.

Between Hastings and Cliff End, to the eastward, the sea has considerably modified the aspect of the cliffs since Mr. Webster described the strata of this coast in detail; and the author of this communication, having devoted much time and labour to the search for fossils in these cliffs, has been enabled to work out the relations of some beds of sandstone and clays subordinate to the conglomeratic shale and ironstone which Mr. Webster described as the lowest strata visible in the series. The above-mentioned shale and ironstone contain remains of Insects (discovered by Messrs. Binfield in 1853) and Saurians, together with *Cyrenæ* and *Unionidæ*; and the ironstone is full of fragmentary plant-remains. In the sandstone beneath are also *Uniones*, and the natural casts of great foot-tracks, already described by Mr. Beckles. In the next succeeding bed are beautiful Zamia-like plant-remains, together with a large *Unio* or *Anodon*, and a *Paludina*. Beneath these is the clay, in which *Hybodus*-spines were the only fossils found.

3. "On the Palæontological and Stratigraphical Relations of the so-called 'Sands of the Inferior Oolite.'" By T. Wright, M.D. Communicated by Prof. Ramsay, F.G.S.

In this communication the author considered the palæontological

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relations of the calcareous sands which lie between the limestone beds of the Inferior Oolite above and the clays of the Lias below, taking the exposures at Leckhampton, Crickley, Frocester, and Symonds Hall Hills as the typical sections. These present (beginning from below) first, the Lower Lias Shale and Limestone; 2. the Marlstone, a hard calcareous sandstone; 3. the Upper Lias Shale, with occasional limestones; 4. yellow and brown fine sands, with bands of concretionary calcareous sandstone; 5. a thin band of calcareo-ferruginous sandstone, abounding with *Cephalopoda*; 6. the pea-grit and other members of the Inferior Oolite; 7. Fullers Earth; 8. Great Oolite. The beds Nos. 4 and 5 constitute the "Calcareous sands" which formed the subject of the paper. Dr. Wright described in detail the several sections alluded to, and enumerated the fossils found in the beds in question. The bed No. 5, called the "Cephalopoda bed," varies from 1 foot to 4 feet in thickness along the edge of the Cotteswolds; and the sands, No. 4, vary considerably, being more than 100 feet in some localities, and thinning away in others. Traced from Cheltenham southwards, these so-called "Inferior Oolite Sands" gradually thicken in the direction of Crickley, Coopers, and Painswick Hills to Beacon, Frocester, and Wootton-under-Edge, where fine sections are exposed.

The author described also sections of these beds in Somersetshire, and at Bridport, Dorset, where they are well developed.

After enumerating the fossil contents of the several beds, Nos. 4, 5, and 6, and pointing out their relations to the faunas of the Oolite and the Lias, Dr. Wright stated it as his opinion that the "Cephalopoda bed" forms an important and well-marked feature in the lower division of the great Oolitic or Jurassic group; and that, although it contains a few species of *Conchifera*, such as *Pholadomya fidicula*, Sow., *Gervillia Hartmanni*, Münst., *Myacites abducta*?, Phil., *Pecten demissus*, Phil., *Modiola plicata*, Sow., *Astarte excavata*, Sow., and *Cucullæa oblonga*, Sow., which are found in the limestones and sands of the Inferior Oolite, still it contains a suite of *Cephalopoda* which are only found in the Upper Lias, and characterise that formation in Germany, France, Belgium, and England. These are *Ammonites opalinus*, Rein., *A. insignis*, Schüb., *A. variabilis*, D'Orb., *A. discoides*, Ziet., *A. striatulus*, Sow., *A. radians*, Schl., *A. Raquienianus*, D'Orb., *A. torulosus*, Schüb., *A. Jurensis*, Ziet., *Nautilus inornatus*, Sow., *Belemnites breviformis*, Ziet., *B. compressus*, Voltz, and *B. Nodotianus*, D'Orb. Dr. Wright considers that the amount of palæontological evidence is in favour of the grouping of the "Cephalopoda bed" and its underlying sands with the Upper Lias rather than with the Inferior Oolite to which it has been considered to belong, and of which it has been described as the basement-bed.

4. "On the probable origin of the English Channel by means of a Fissure." By M. Ami Boué, For. Mem. G.S.

The author, having met with a published proposal to construct a submarine tunnel across the Straits of Dover, pointed out that it was highly probable that the English Channel had not been exca-

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vated solely by water-action, but owed its origin to one of the lines of disturbance which have fissured this portion of the earth's crust, and that, taking this view of the case, the fissure probably still existed, being merely filled with comparatively loose material, and would prove a serious obstacle to any attempt to drive a submarine tunnel which would have to traverse it.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 310.]

May 7, 1855.—The Master of Trinity read a paper on Plato's notion of *Dialectic*.

At the end of the survey of the sciences contained in the seventh book of the Republic, which was the subject of a paper at the last meeting, Plato speaks of *Dialectic* as a still higher element of a philosophical education, fitted to lead men to the knowledge of real existences and of the Supreme Good. Here he describes Dialectic by its objects and purpose. In other places Dialectic is spoken of as a method or process of analysis; as in the 'Phædrus,' where Socrates describes a good dialectician as one who can divide a subject according to its natural members, and not miss the joint, like a bad carver. Another Dialogue, in which there are examples given of dividing a subject, is the *Sophistes*, where many examples of dichotomous or bifurcate division are given. But this appears from the Dialogue to have been a practice of the Eleatic rather than of the Platonic school. Aristotle proposed a division of subjects according to his ten *Categories*, which he and others since have extensively used. Xenophon says that Socrates derived *Dialectic* from a term implying to *divide a subject into parts*, which Mr. Grote thinks unsatisfactory as an etymology, but which has indicated a practical connexion in the Socratic school. The result seems to be, that Plato did not establish any method of analysis of a subject as his Dialectic; but he conceived that the analytical habits formed by the comprehensive study of the exact sciences, and sharpened by the practice of dialogue, would lead his students to the knowledge of first principles.

Also, Mr. Maxwell gave an account of some experiments on the mixture of colours.

May 21.—A paper was read by Mr. Hopkins on the External Temperature of the Earth and the other Planets of the Solar System.

We have not sufficient *data* to determine the superficial temperature of any planet besides our own. We know, however, that it must mainly depend on the temperature of the planetary space, and on the heat which the nearer planets at least receive directly from the sun, but modified, and possibly in a far greater degree than has been generally supposed, by the particular circumstances by which each planet may be characterized. The modifying circumstances more particularly referred to in this paper, are the existence of atmospheres surrounding the planets, the positions of their axes of rotation, and the conductivity and specific heat of the substances forming the outer crust of each planetary body of our system. No

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astronomer, judging from the appearances which Mars and Jupiter present to us, would entertain any serious doubt as to the existence of atmospheres surrounding those planets, and the probability would seem to be almost equally strong of Saturn being likewise enveloped in a similar manner. The obliquity of the axis of rotation is known with considerable accuracy in the cases of Mars and Jupiter; and also in that of Saturn, if it coincide with the axis of rotation of his ring. Venus presents great difficulties to the observer, but it appears now to be pretty satisfactorily determined, that the period of rotation about her own axis is nearly the same as that of the Earth, and that the obliquity of her axis is large, amounting to as much as about 75°. This must produce an extraordinary difference between the changes of *annual* temperature in that planet and those which we experience. The author has endeavoured, in this paper, to estimate numerically the effect of this anomalous obliquity. Practical astronomers have entertained the opinion that Venus likewise has an atmosphere. Of Mercury we know too little by direct observation to form any opinion on those points founded on observed facts, and the same remark will apply to the remoter planets beyond Saturn; but most astronomers probably feel much the same conviction that Mercury, Uranus, and Neptune have atmospheres of greater or less extent, as that they revolve round their own axes with greater or less angular velocity.

It is not the author's object, however, to adjust the balance of probabilities for particular hypotheses in favour of planetary atmospheres or against them; but assuming their existence, to estimate their effects on the planetary temperatures. And in like manner he points out the influence which must be exercised by a greater or less conductivity, and specific heat in the superficial matter of a planet, without professing to discuss the probability of such properties being materially different in the different planets. The Earth's atmosphere is known to be almost completely diathermanous for heat radiating directly from the Sun; and it is assumed to be equally so for the heat which proceeds directly from the fixed stars, and to which the general temperature of space is due. This radiating heat therefore has little or no effect in heating the atmosphere during its transmission to the Earth's surface; but after falling upon and heating terrestrial objects, it loses the power of radiating completely through the atmosphere, and is transmitted back into space through the atmosphere by *conduction*, *convection*, and *partial radiation* to limited distances. But for any of these modes of transmission, it is essential that the temperature of the atmosphere should be greater in its lower than in its upper portions, and in a degree greater as the quantity of heat to be transmitted is greater. The temperature (τ_2) of the upper portion must be determined by the condition, that, in a given time, a quantity of heat must radiate from it into surrounding space equal to that which falls upon it from external sources in the same time, and is transmitted back after reaching the surface of the earth or objects near to it. Consequently τ_2 must be independent of the height of the Earth's atmosphere. At lower points the temperature will increase till we reach the surface of the Earth; and if we denote the tempe-

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rature there by τ_1, it is manifest that τ_1 will be greater, the greater the height of the Earth's atmosphere.

It must here be particularly observed, that τ_2 is the proper temperature of the component particles of the atmosphere, and is probably widely different from the temperature which would be indicated by a thermometer placed at the upper extremity of the atmosphere, since the instrument would not only be affected by the exchange of heat between its bulb and the atmospheric particles, but also by the heat radiating upon its bulb from every source of heat in surrounding space; while the atmosphere, on account of its diathermancy, would remain unaffected by this radiating heat.

Conceive now a thermometer to be placed at a point sufficiently above the earth's atmosphere. If the bulb were sheltered from the direct influence of the solar rays, the thermometer would indicate the temperature of that point of space, independent of the effect of radiation from the central luminary of the solar system, but dependent on the radiation from all other sources of heat in the universe. If the instrument thus sheltered were sufficiently remote from the sun and every planet, it would indicate very nearly the same temperature at every point within the solar system, assuming the absence of all unknown centres of heat within that system or near to it. This is what may be understood by the general temperature of planetary space. Let it be denoted by T. We shall then have T greater than τ_2; and therefore if we now conceive the thermometer to be transported to the upper limit of the atmosphere, it will be affected by the lower temperature there, and will indicate a temperature intermediate to T and τ_2. If the instrument be brought still lower within the atmosphere, it will indicate a still lower temperature, from its being entirely surrounded by a portion of the atmosphere more dense than that at the extreme boundary, till this tendency to lower the indications of the thermometer is counteracted by the greater temperature of the atmospheric particles as we descend towards the Earth's surface. At some point, consequently, within the Earth's atmosphere the indication of the thermometer would attain its *minimum*; after which, in descending continuously towards the Earth, the temperature indicated would constantly increase, omitting variations due to temporary or local causes. Thus it follows that the existence of an atmosphere like that of the Earth, enveloping a planet, may, according to its extent, either elevate the superficial temperature of the planet above, or depress it below that of surrounding space independently of the direct solar radiation. With respect to our own globe, we are entirely ignorant of the height to which the thermometer, in ascending, would continue to indicate a decreasing temperature, but we are sure that such height is great. This is important with reference to the ultimate object of this paper; for if the height of a planet's atmosphere were too small to allow a thermometer descending in it to attain its *minimum* indication, it is manifest that an increase of atmosphere would cause a *decrease* in the planet's superficial temperature; whereas if the height of the atmosphere were great enough to allow the thermometer to attain the *minimum*, any increase of atmosphere would necessarily cause an *increase* in

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the superficial temperature of the planet. In the Earth's atmosphere, we are sure (as just remarked) that the indications of a thermometer would constantly increase in its descent from a very high point above the Earth's surface; and therefore it follows, that if a planet be enveloped in an atmosphere similar to that of the Earth, but of greater height, the superficial temperature of that planet will be higher than that of the Earth, supposing both to exist in the planetary space unaffected by the heat which radiates from the Sun; while, on the contrary, the superficial temperature of the planet would necessarily be less, under the same conditions, than that of the Earth, if its atmosphere were smaller, unless it should be so small as not to allow a thermometer descending in it to reach its *minimum* indication. If the planet were entirely without atmosphere, its superficial temperature (in the assumed absence of solar radiation) would be that of surrounding space; but we have no means of determining what relation that temperature bears to existing terrestrial temperature, or to what this latter temperature would become in the absence of solar radiation.

The author has calculated from Poisson's formulæ the increase of temperature in the superficial crust of the Earth, due to the amount of heat received by direct radiation from the Sun, in different latitudes, above that temperature which would be common to all parts of the Earth's surface in the absence of solar radiation, and with a uniformity of intensity of stellar radiation in all directions upon our globe. But this increased temperature must produce an augmentation of temperature in the atmosphere, which must react on the terrestrial temperature till equilibrium of temperature be established. The author has endeavoured to estimate the amount of this indirect effect of solar radiation by means of the data furnished by M. Dove's work on terrestrial temperatures, combined with calculations based on Poisson's formulæ. He concludes that the whole effect of solar heat at any proposed place is very nearly double that due to the immediate and direct effect of solar radiation. Having thus ascertained this entire effect, he finds the temperature which would pervade the whole surface of the earth if the solar heat were extinguished. He estimates this temperature at $-39°{\cdot}5$ C.

The *annual variation* of temperature in any latitude is found to be nearly the same in amount for the terrestrial surface and for the part of the atmosphere resting upon it. This must be understood as applying to those places at which the temperature is not materially affected by the *horizontal* transference of heat by marine or aërial currents, or any local causes, which disturb the dependence of temperature on latitude alone. The author also points out the dependence of the annual inequalities of the terrestrial temperature (and consequently of those also of the atmosphere) on the conductivity and specific heat of the matter which constitutes the Earth's crust. If these were much greater, the annual changes of temperature would be much less.

Before applying these results to other planets, the author states that he does not admit the notion, that the remoter planets may derive a considerable superficial temperature from the remains of that in-

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ternal heat which they probably possessed in the earlier stages of their existence. It is a well-established conclusion, that the superficial temperature of our own globe has arrived at that point below which it can never descend by more than the small fraction of a degree, so long as all *external* conditions remain the same as at present; and the superficial temperature of the remoter planets will in all probability be reduced to the corresponding limit. To these external conditions, therefore, and not to their primitive heat, must the existing temperatures on the surfaces of these planets be attributed, assuming always that they are not of less antiquity than our own globe. Hence the superficial temperature of the Earth, with its present atmosphere, placed at the distance of Neptune, Uranus, or Saturn, would be very nearly $-39°{\cdot}5$ C., since the effect of solar radiation at those distances would be nearly insensible. But if the extent of the atmosphere were increased, the superficial temperature would be augmented in a corresponding degree. Judging by the decrements of temperature observed by Mr. Welsh, the author concludes that an increase in the height of the Earth's atmosphere of 35,000 or 40,000 feet, would elevate her superficial temperature, if placed in the remoter planetary regions, to nearly the mean temperature of our present temperate zone. The same conclusion will hold with respect to the three planets above mentioned, if we suppose them to have atmospheres similar to that of the Earth, and of sufficient extent. Their temperatures must be sensibly uniform over the whole of their surfaces, not being subject to any appreciable *annual* variation.

The same conclusions will apply to Jupiter, except that there will be a small augmentation of temperature arising from solar radiation, which the author calculates might amount to about $2\frac{1}{2}°$ C. at his equator.

Hence the author concludes that those views which assign a necessarily low temperature to the above-mentioned planets in consequence of their distance from the Sun, are altogether untenable.

The conditions under which Mars is placed approximate more nearly to those of the Earth than for any other planet. The author calculates, that with an atmosphere similar to that of the Earth, and exceeding it in height by about 15,000 or 20,000 feet, the equatorial temperature of Mars may be about 60° F., or $15°{\cdot}5$ C., and his polar temperature about $-10°$ C. The extent of the *annual* variations would be about half those on our own planet in corresponding latitudes, supposing the conductivity, specific heat, and radiating power of the matter composing his superficial crust to be the same as for the Earth.

Again, if the Earth, with her present atmosphere and *obliquity*, were placed in the orbit of Venus, the mean equatorial temperature would be upwards of 90° C., subject to the reduction, which would doubtless in this case be great, due to the horizontal transference of heat. The mean polar temperature would be about 16° C. A diminution in the atmosphere would reduce these temperatures in any assigned degree. But the *obliquity* of Venus, though not satisfactorily determined, is considered to be much greater than that of

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the Earth, amounting, according to the estimate of some astronomers, to as much as 75°, as heretofore stated. This would of course render the character of her seasons entirely different from those of the Earth. The greatest mean annual temperature would be at the pole. Independently of the horizontal transference of heat by aërial currents or other causes, taking the extreme obliquity of 75°, and supposing the atmosphere of Venus to be exactly like that of the Earth, her mean temperature at the equator would be about 56° C., and at the pole 95° C. This latter would probably be much lowered by currents; but if the height of the atmosphere of Venus be less than that of the Earth's atmosphere by about 25,000 feet, the author considers that the mean temperature of Venus in her equatorial regions would not exceed that of the temperate regions of the Earth; while the mean polar temperature would probably be about 40° C., or about 12° or 13° C. higher than the Earth's equatorial temperature. The heat of *sunshine* may be moderated by an atmosphere more laden with vapour than that of the Earth.

Supposing the atmosphere of Venus like that of the Earth in its nature and its magnitude, the temperature at her poles, with the supposed obliquity, must be subject to an enormous *annual* inequality, amounting to between 70° and 80° C. above or below the mean temperature, liable, however, to a great reduction by horizontal transference of heat. It may also be considerably reduced by the nature of the matter which constitutes her outer crust. A reduction, likewise, in the extent of her atmosphere, like that above supposed, would probably diminish the amount of this inequality, as well as the mean temperature, though not in the same degree. It is easy to conceive that the coefficient of the inequality may be thus reduced to some 40° C.; and supposing the mean temperature then, as above estimated, at about 40° C., the *annual* polar temperature will oscillate between 0° C. and 80° C. At the equator, the *semi-annual* inequality might amount, under the above suppositions, to about 10° or 12° C., in which case the equatorial temperature might oscillate between something below zero (C.) and some 25° C. It should be recollected also, that a much greater reduction of the mean temperature would result from a greater reduction in the extent of this planet's atmosphere than above supposed with reference to the height of our own atmosphere. This would not, the author conceives, be inconsistent with the existence of a large quantity of vapour in the atmosphere, affording shelter from the heat and glare of sunshine.

The Moon is under the peculiar circumstances of the absence of a sensible atmosphere, and her long period of rotation about her axis. Assuming her to have no atmosphere at all, the mean temperature of her outer crust, in the absence of the Sun, would be the general temperature of that portion of planetary space in which the solar system is situated. How much this might differ from the superficial temperature which the Earth would have with the like absence of the Sun, and which the author estimates at −39°·5 C., as above stated, it is impossible to determine; but whatever it may be, the influence of the Sun's heat would be to increase it by about 40° C. at the Moon's equator, and by a small amount only at her

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poles. This must be attended by an enormous monthly inequality, amounting to nearly 60° C., supposing the matter of which her superficial crust is composed to have the same conductivity, specific heat, and radiating power as the crust of the Earth. If these be much greater for the Moon, this inequality might be considerably diminished. At the poles it must be comparatively small.

The lunar temperatures here spoken of are those of the matter forming her external crust. The temperature which would be indicated by a thermometer placed in her immediate vicinity would be affected by the Moon (in the assumed absence of an atmosphere) only by her direct radiation. We have not the means of determining what this temperature may be.

Nov. 12.—A paper was read by the Master of Trinity on the Intellectual Powers according to Plato.

Also, Prof. Sedgwick gave a lecture on the Classification and Nomenclature of the Palæozoic Rocks.

Dec. 10.—A paper was read by Mr. Maxwell on Faraday's Lines of Force.

The method pursued in this paper is a modification of that mode of viewing electrical phænomena in relation to the theory of the uniform conduction of heat, which was first pointed out by Professor W. Thomson in the Cambridge and Dublin Mathematical Journal, vol. iii. Instead of using the analogy of heat, a fluid, the properties of which are entirely at our disposal, is assumed as the vehicle of mathematical reasoning. A method is given by which two series of surfaces may be drawn in the fluid so as to define its motion completely. The uniform motion of an imponderable and incompressible fluid permeating a medium, whose resistance is directly as the velocity, is then discussed, and it is shown how a system of surfaces of equal pressure may be drawn, which, with the two former systems of surfaces, divides the medium into cells, in each of which the same amount of work is done in overcoming resistance. It is then shown that if the fluid be supposed to emanate from certain centres, and to be absorbed at others, the position of these centres can be found when the pressure at any point is known; and that when the centres are known, the distribution of pressures may be found. Methods are then given by which the motion of the fluid out of one medium into another, the resistance of which is different, may be conceived and calculated, and the theory of motion in a medium in which the resistance is different in different directions is stated.

The mathematical ideas obtained from the fluid are then applied to various parts of electrical science. It is shown that the expression for the electrical potential at any point is identical with that of the pressure in the fluid, provided that "sources" of fluid are put instead of positive electrical "matter," and centres of absorption or "sinks" for negative "matter."

The theory of Faraday with respect to the effect of dielectrics in modifying electric induction, is illustrated by the case of different media having different conducting power; and it is shown, that, in order to calculate the effects by the ordinary formulæ of attractions,

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we must alter in a certain proportion the quantities of electricity within the dielectric, and conceive an imaginary distribution of electricity over the surface which separates it from the surrounding medium.

The theory of magnets and of the phænomena of paramagnetic and diamagnetic bodies is expressed with reference to the "lines of inductive magnetic action;" and elementary proofs of the tendency of paramagnetic bodies toward places of stronger magnetic action, and of diamagnetic bodies toward places of weaker action, are given. This distinction of paramagnetic and diamagnetic is not here used absolutely, but indicates a greater or less conductivity for the lines of inductive action than that of the surrounding medium.

The magnetic phænomena of crystals are then examined, and referred to unequal magnetic conductivity in different directions; and the case of a crystalline sphere in a uniform field of force is worked out.

The laws of electric conduction, as laid down by Ohm, are shown to agree with those of the imaginary fluid, and definitions of quantity and intensity are given, which will apply to magnetism as well as galvanism.

The theory of the attractions of closed circuits, as established by Ampère, is shown to lead to the following results:—

1. The total intensity of the magnetizing force estimated along any closed curve embracing the circuit is a measure of the quantity of the current.

2. The quantity of the current, multiplied by the quantity of inductive magnetic action, from whatever source, which passes through it, gives what may be called the potential of the circuit. The tendency of the resultant forces is to increase this potential.

The theory of Faraday with respect to the induction of currents in closed circuits takes the following form:—

When the quantity of inductive magnetic action which passes through a given circuit changes in any way, an electromotive force proportional to the rate of change acts in the circuit, and a current is produced whose quantity is the electromotive force divided by the total resistance of the circuit.

The mathematical discussion of the electro-magnetic laws is reserved for another communication.

LI. *Intelligence and Miscellaneous Articles.*

ON VÖLKNERITE OR HYDROTALKITE, AND THE SO-CALLED STEATITE OF SNARUM. BY C. RAMMELSBERG.

THE author finds that the minerals described by Hochstetter as hydrotalkite, and by Hermann as völknerite, are identical, as indeed was supposed by Hermann. The masses examined by the author are in crooked laminæ, and divide, when struck, into crooked parallel fibres; they are somewhat flexible, but may be powdered. Spec grav. =2·091. Their analyses gave,—

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	I.	II.	III.	IV.
Carbonic acid....	2·61	6·05	7·32	7·30
Magnesia	37·27	38·18	37·30	37·04
Alumina.....	19·25	17·78	18·00	18·87
Water	41·59	(37·99)	(37·38)	37·38
	100·72	100·00	100·00	100·59

Like Hermann, the author is of opinion that the carbonic acid is to be regarded as belonging to a carbonate subsequently produced; and if this be deducted, the above numbers lead to the following proportions of oxygen:—

	Al^2O^3.	MgO.	HO.	
In I.	=8·99 :	14·91 :	36·97	= 3 : 5·0 : 12·3
In II.	=8·31 :	15·27 :	33·78	= 3 : 5·5 : 12·2
In III.....	=8·40 :	14·92 :	32·23	= 3 : 5·3 : 11·9
In IV.....	=8·81 :	14·81 :	33·23	= 3 : 5·0 : 11·3
Hermann....	=7·92 :	14·83 :	37·37	= 3 : 5·6 : 14·1

Hermann has assumed the proportion of oxygen to be =3 : 6 : 15. The average of the author's analyses is 3 : 5·2 : 11·9, for which we may substitute 3 : 5 : 12. According to this, völknerite may be regarded as MgO, Al^2O^3+4(MgO, 3HO), or as

$$Al^3O^3, 3HO+5(MgO, 2HO).$$

Calculation gives for—

	MgO Al^2O^3+4(MgO, 3HO).	Al^2O^3, 3HO+5(MgO, 2HO).
Magnesia	38·56	37·27
Alumina	19·80	19·14
Water	41·64	43·59

But the analyses, after deducting the carbonic acid, give,—

	I.	II.	III.	IV.	Hermann.
Magnesia ..	38·27	40·64	40·25	40·00	38·59
Alumina ..	19·75	18·92	19·42	20·35	17·65
Water	42·70	40·44	40·33	40·24	43·76
	100·72	100·00	100·00	100·59	100·00

Steatite.—In both the places where völknerite occurs it is accompanied by a compact mineral which has usually been characterized as steatite, but which has also been taken sometimes for talc, and sometimes for mica. It is of a gray or greenish colour, fatty to the touch, very tough, and without distinct structure. According to Hermann, it has a specific gravity of 2·50, and occurs as pseudomorphs of garnet, and perhaps of epidote. Under the name of mica from Snarum, the author received a mineral of a greenish colour and of a laminar texture; it furnished on analysis,—

			Oxygen.
Silica.....	34·88		18·12
Alumina	12·48	5·83	7·57
Oxide of iron ..	5·81	1·74	
Magnesia	34·02		13·37
Water	13·68		12·16
	100·87		

from which it would appear to be identical with steatite.

The quantities of oxygen in the constituents of steatite from the

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Ural, analysed by Hermann, are nearly equal according to his analysis; according to the author's, their proportion in $SiO^3 : R^2O^3 : MgO : HO$ is rather as 3 : 1 : 2 : 2. Consequently steatite from the Ural would be $(MgO)^6, SiO^3 + (Al^2O^3)^2, SiO^3 + 6HO$. The Norwegian steatite, on the contrary, would be $2(MgO)^3, SiO^3 + Al^2O^3, SiO^3 + 6HO$. This formula is the same as that deduced by Hartwall from his analysis for the Kämmererite of Bissersk. — Poggendorff's *Annalen*, vol. xcvii. p. 296.

PREPARATION OF PEROXIDE OF LEAD BY MEANS OF FERRIDCYANIDE OF POTASSIUM. BY DR. A. OVERBECK.

The solution of ferridcyanide of potassium is boiled with hydrated oxide of lead and potash until the oxide of lead is converted into peroxide. The solution which remains after the conversion furnishes ferrocyanide of potassium.

$$KO + PbO + K^3 Fe^2 Cy^6 \text{ give } PbO^2 + 2(K^2 Fe Cy^3).$$

—*Archiv der Pharmacie*, vol. lxxxv. p. 5.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1856.

Chiswick.—March 1. Cloudy: fine. 2. Cloudy: slight rain. 3, 4. Cloudy and cold. 5. Overcast. 6. Cloudy and cold: fine. 7. Cloudy: fine. 8. Fine: slight rain. 9. Cloudy. 10. Foggy: fine. 11. Foggy: hazy: frosty at night. 12. Clear and frosty: cloudy and windy. 13. Cold and dry. 14. Excessively cold wind: clear, cold and dry. 15. Cloudy. 16. Slight haze: heavy rain at night. 17. Hazy. 18. Hazy: fine rain. 19, 20. Hazy: overcast. 21. Hazy: cloudy. 22. Overcast: fine. 23. Hazy: fine: clear. 24, 25. Hazy and cold. 26. Cloudy and cold. 27. Clear: fine: frosty. 28. Cloudy and cold. 29. Dry cold haze: frosty at night. 30. Slight haze: fine: sharp frost. 31. Slight haze: very fine: frosty.

Mean temperature of the month	38°·95
Mean temperature of March 1855	37 ·61
Mean temperature of March for the last thirty years	42 ·09
Average amount of rain in March	1·344 inch.

Boston.—March 1, 2. Cloudy. 3. Cloudy: rain P.M. 4, 5. Cloudy. 6. Cloudy: rain A.M. 7—9. Fine. 10. Cloudy. 11. Cloudy: snow A.M. 12, 13. Cloudy. 14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy: rain A.M. and P.M. 18. Cloudy: rain P.M. 19. Cloudy: rain A.M. 20. Cloudy. 21. Cloudy: rain A.M. and P.M. 22—26. Cloudy. 27. Fine. 28. Cloudy. 29—31. Fine.

Sandwick Manse, Orkney.—March 1. Cloudy A.M.: fine, drizzle P.M. 2. Drizzle A.M.: fine, cloudy P.M. 3. Damp A.M.: fine, clear P.M. 4. Showers, fine A.M.: fine, cloudy P.M. 5. Damp A.M.: fine, aurora P.M. 6. Drops A.M.: fine P.M. 7. Fog A.M. and P.M. 8. Showers A.M.: fine, aurora P.M. 9. Showers A.M.: cloudy P.M. 10. Frost A.M.: clear, frost P.M. 11, 12. Snow-showers A.M. and P.M. 13. Snow, frost A.M.: clear, frost P.M. 14. Clear, frost A.M.: very clear P.M. 15. Bright A.M.: very clear, aurora P.M. 16. Clear A.M.: very clear P.M. 17. Bright A.M.: cloudy P.M. 18. Cloudy A.M. and P.M. 19. Bright A.M.: cloudy P.M. 20, 21. Cloudy, fine A.M. and P.M. 22. Bright, fine A.M.: clear, fine P.M. 23, 24. Cloudy A.M.: cloudy, fine P.M. 25. Cloudy A.M.: clear, fine P.M. 26. Bright A.M.: cloudy, fine P.M. 27. Bright A.M.: clear, aurora P.M. 28. Cloudy A.M.: very clear, fine P.M. 29. Clear A.M.: very clear, fine P.M. 30. Cloudy A.M.: very clear, fine, aurora P.M. 31. Bright A.M.: cloudy, fine P.M.

Mean temperature of March for previous twenty-nine years...	40°·40
Mean temperature of this month	40 ·39
Mean temperature of March 1855	36 ·61
Average quantity of rain in March for fifteen previous years .	2·60 inches.

This month has been unprecedentedly dry, the rain being only about one-eighth of the average for March, and less than that of any month during my observations, except April 1852, when it was only ·11 of an inch. Rain fell only on eight days.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¼ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¼ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1856. March.	Max.	Min.		9½ a.m.	8½ p.m.	Max.	Min.		9½ a.m.	8½ p.m.						
1.	30·606	30·584	30·24	30·50	30·54	44	40	41	46	45½	ne.	n.	w.	.....	.....	.....
2.	30·569	30·556	30·20	30·59	30·64	46	37	45	47½	43	ne.	ne.	w.	·01	.....	·04
3.	30·577	30·530	30·22	30·61	30·55	44	35	41·5	43½	42	ne.	n.	wsw.	.....	.....	.....
4.	30·539	30·475	30·18	30·43	30·37	44	33	38·5	44	44½	ne.	n.	w.	.....	·02	·04
5.	30·286	30·177	29·95	30·20	30·26	46	33	35	47	41	ne.	n.	nnw.	.....	.....	.....
● 6.	30·197	30·139	29·84	30·27	30·32	44	27	40	40	40½	ne.	n.	w.	·01	·02	·01
7.	30·350	30·230	29·98	30·28	30·25	44	21	36·5	46	44	ne.	n.	wnw.	.....	.....	.....
8.	30·341	30·297	29·98	30·20	30·20	52	26	34·5	46	44½	se.	nw.	w.	.....	.....	·07
9.	30·232	30·131	29·80	29·92	30·02	51	25	38	46	37½	w.	w.	nw.	.....	.....	·02
10.	30·126	30·079	29·73	30·09	30·18	51	35	43·5	34½	30	n.	ne.	.....	.....	.....	·01
11.	30·080	29·983	29·77	29·87	30·05	44	19	40	35	29	e.	s.	nnw.	.....	.....	·05
12.	30·006	29·944	29·70	30·09	30·08	43	31	38	32	32	ne.	ne.	s.	.....	·02	·07
☽ 13.	30·070	29·988	29·80	30·10	30·27	42	31	36	36	34	e.	e.	s.	.....	.....	·03
14.	30·140	30·103	29·95	30·39	30·46	43	28	36	38	36	e.	e.	se.	.....	.....	.....
15.	30·144	30·090	29·94	30·42	30·36	46	32	36	38½	36½	e.	e.	ese.	.....	.....	.....
16.	30·023	29·949	29·75	30·23	30·15	50	39	39	42	40	e.	e.	se.	·68	.....	.....
17.	29·951	29·924	29·67	30·06	30·01	44	39	42	43	41	e.	ese.	se.	·02	.....	.....
18.	29·930	29·764	29·61	30·05	30·03	50	41	44·5	43	39	e.	e.	se.	·24	·12	.....
19.	29·909	29·734	29·40	29·91	29·93	51	41	46·5	42½	39	ne.	ese.	e.	.....	·02	.....
20.	29·953	29·932	29·62	29·98	30·01	52	40	41·5	44	41	n.	n.	ne.	·01	.....	.....
○ 21.	29·985	29·896	29·56	30·00	30·04	54	30	43	44	40	n.	n.	e.	.....	.....	.....
22.	30·063	30·051	29·74	30·12	30·19	51	25	46	44	39	n.	nw.	.....	.....	·05	.....
23.	30·148	30·074	29·86	30·25	30·27	50	35	42	42½	40	ne.	n.	nne.	.....	.....	.....
24.	30·144	30·007	29·85	30·22	30·17	45	36	41	42	40	e.	ne.	.....	.....	.....	.....
25.	29·909	29·828	29·62	30·11	30·11	43	35	40	41½	38½	ne.	ne.	e.	.....	.....	.....
26.	29·879	29·852	29·66	30·14	30·12	42	29	39·5	44	37	e.	e.	sse.	.....	.....	.....
27.	29·884	29·873	29·68	30·04	30·06	51	21	36·5	38	37	ne.	ne.	nne.	.....	.....	.....
28.	29·979	29·913	29·68	30·09	30·19	47	29	40	42	33	ne.	n.	e.	.....	.....	.....
☾ 29.	30·232	30·144	29·92	30·19	30·17	44	17	36	41	40	e.	e.	wsw.	.....	.....	.....
30.	30·243	30·212	29·96	30·08	30·02	52	15	32	41	36	e.	s.	ese.	.....	.....	.....
31.	30·152	30·018	29·86	29·86	29·77	60	20	32	44	45½	s.	se.	s.	.....	.....	.....
Mean.	30·111	30·079	29·83	30·170	30·186	47·42	30·48	39·4	41·88	38·90				0·97	0·25	0·34

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JUNE 1856.

LII. *Contributions to the Metallurgy of Copper.* By A. DICK*.

THE following investigation was made in the metallurgical laboratory of the Museum of Practical Geology, at the request of Dr. Percy, with the special object of ascertaining the causes of the characteristic properties of ordinary "tough pitch copper" and of "overpoled copper†."

Before detailing the experiments, it may be well to describe very briefly that part of the smelting process termed refining, in which the copper is obtained in these conditions. An impure copper, the product of previous operations, is kept melted in the oxidizing atmosphere of a reverberatory furnace for a considerable time. The products are copper containing suboxide in solution, and a slag rich in suboxide of copper. The object of this process is to remove as completely as possible by oxidation, the last traces of various metals and the sulphur left in the copper after the previous treatment. Copper thus saturated with suboxide is known as *dry copper*. The slag is then skimmed off, and anthracite thrown upon the surface of the melted copper. By this means the suboxide is reduced, and the action is completed by plunging one end of a pole of green wood under the surface of the melted metal. The gases produced by the decomposition of the wood produce a kind of ebullition, which causes every portion of the metal to be brought more rapidly and thoroughly in contact with the anthracite than would otherwise

* Communicated by the Author.

† For a detailed description of the refining of copper see Phil. Mag. S. 4. vol. v. p. 406.

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be the case. When the copper has attained its maximum toughness and malleability, it is laded into ingot moulds, and is known as "tough pitch copper." If the poling process is continued beyond a certain point, the metal loses much of its toughness and malleability, and is known as "overpoled copper."

DRY COPPER.—*Determination of the amount of suboxide.* The sample operated upon was made at the Hafod Works, Swansea, in the year 1848, in the presence of Dr. Percy.

(*a*). *By heating a known weight of dry copper in hydrogen and weighing the water produced.*—Some of the metal cut from the ingot was rolled out as fine as possible and cut into small pieces, of which 132·34 grs. were placed in a German glass tube, which was connected with a weighed tube containing chloride of calcium. A current of dry hydrogen was then passed through the tubes, and, after complete expulsion of the air, heat was applied to that containing the copper. When the temperature rose to redness, a distinct odour of sulphuretted hydrogen was perceived in the escaping gas, which instantly blackened lead paper. This production of sulphuretted hydrogen is curious, because it shows that the copper saturated with suboxide still retains a trace of sulphur in some form or other. During the progress of the experiment, a slight metallic sublimate was formed in the cooler part of the tube—but in a part much too hot for arsenic to condense,—which was found to contain lead. The quantity was, however, too small for any very exact experiments to be made upon it. After continuing the experiment for some time, the tube containing chloride of calcium was disconnected, and when cold reweighed. The weight of water was 1·93 gr., which is equivalent to 10·21 per cent. of suboxide in the dry copper. In another experiment, 136·41 grs. of the same dry copper gave 1·82 gr. of water, which is equivalent to 9·34 per cent. of suboxide. Sulphuretted hydrogen was again observed at the beginning of the experiment. It is certain that the hydrogen employed was free from sulphuretted hydrogen and also from water. The difference in the results shows that the method cannot be relied on for accuracy.

(*b*). *By melting a known weight of dry copper in hydrogen and estimating the oxygen by loss.*—The apparatus employed was a small Stourbridge clay crucible fitted with a perforated lid. It was filled with hydrogen by means of a small porcelain tube which passed through the perforation in its lid, and was heated by charcoal arranged in a convenient furnace. Satisfactory results were not obtained; because, when the dry copper was melted in this way, it was found that great spirting occurred, and that the projected globules could not be all collected from

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the inside of the lid and crucible for the purpose of being weighed. This took place even when the heat was applied very slowly, and was apparently due to the escape of the water formed by the reduction of the suboxide.

(*c*). *As loss by the wet way.*—A weighed portion of dry copper was dissolved in nitric acid, and the solution was saturated with caustic potash and boiled. The precipitate was collected on a filter, washed, ignited, and weighed. It was evaporated with nitric acid until its weight was constant. From the oxide obtained the metal was calculated, the difference between the weight of which and that of the dry copper was estimated as oxygen. This method will not yield absolutely accurate results, because dry copper is not a mixture of chemically pure copper and suboxide, but contains in addition small quantities of lead, antimony, and other metals, which will interfere slightly with the result, owing to the difference in their atomic weights: still the error must be very small. In one experiment, 10·73 grs. of dry copper yielded 13·18 grs. of oxide; in another, 9·17 grs. of dry copper yielded 11·26 grs. of oxide. From the first experiment, the dry copper contained 98·09 per cent. of copper; from the second, it contained 98·01 per cent. The difference estimated as oxygen corresponds to 17·04 per cent. of suboxide of copper by the first experiment, and 17·74 per cent. by the second.

Tough Pitch Copper.—In this condition, the metal, like chemically pure copper in certain states, is possessed of the highest degree of malleability and ductility at all temperatures. It is well known that tough pitch copper cannot be remelted, except under special conditions, without losing in part its malleability. Karsten showed that it contained suboxide of copper, and that this was essential to counteract the injurious effect exerted upon the malleability of the metal by foreign metals. Hence it is easy to conceive why tough pitch copper can scarcely be remelted without losing part of its malleability; because, if the atmosphere is a reducing one, the suboxide is reduced, and the metal assumes the brittleness of overpoled copper; or, if it is an oxidizing one, too much suboxide is formed, and the metal assumes the brittleness of dry copper. The following experiments were made upon this subject. The tough pitch copper operated upon was either an ingot made at the Hafod Works, Swansea, or else commercial wire or sheet.

Tough pitch copper from the ingot, after having been melted in hydrogen, was so brittle that it split at once when hammered at the ordinary temperature, and the brittleness was much greater when the metal was hot. Various samples of wire and sheet, similarly treated, were all found more or less brittle, especially when hammered hot. Similar samples melted under charcoal

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were similarly altered; and that this was not due to carbon absorbed by the metal and acting injuriously upon its malleability, will be shown fully further on; but it may be here stated, that electrotype copper, which is nearly chemically pure copper—at least that operated upon was,—remained perfectly malleable after having been melted under charcoal*. The charcoal employed in these and all following experiments had been digested in hydrochloric acid and thoroughly washed with water, to get rid as much as possible of the action of the ash upon the metal in presence of carbon.

Demonstration of the presence of suboxide in tough pitch copper.

(*a*). *By heating a known weight in hydrogen and weighing the water produced.*—The metal was heated to redness in hydrogen, when water was found to be produced. Experiments were then made to determine the proportion of suboxide in the same way as already described (*a*) in respect to dry copper, and the results obtained were equally discordant and unsatisfactory. Towards the beginning of each experiment a trace of sulphuretted hydrogen was produced, just as when dry copper was similarly operated on; and it was ascertained that tough pitch copper does contain a trace of sulphur, by dissolving it in nitric acid and testing the solution for sulphuric acid. A small metallic sublimate containing lead was formed, as when dry copper was experimented on. The highest amount of suboxide shown by this method was 2·95 per cent.

When copper wire or foil was so treated, it was found to undergo a curious change. After having been heated in hydrogen, it could not be bent without at once breaking, and it lost its lustrous surface. The pliability of the wire or foil could not be restored by annealing it at a red heat in steam, which was employed because it exerts neither an oxidizing nor a reducing action. The same loss of pliability took place when carbonic oxide or coal-gas was employed instead of hydrogen. It evidently arises from the porosity produced by the reduction of the suboxide which the metal contains, and must be distinguished from the brittleness produced by *melting* the wire or foil in any of these gases. For if the metal be first melted in any of them and then rolled out, which can be done though the metal cracks slightly at the edges, it will be found that the foil so produced is not in the least affected by heating in them; moreover, electrotype copper, which contains no suboxide, is not altered by heating in any of them.

(*b*). *By melting a known weight of copper wire in hydrogen and estimating the oxygen by loss.*—The same spirting took place as

* The copper employed was prepared by Messrs. Elkington and Co., Birmingham.

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when dry copper was operated upon, though in a less degree, but still sufficient to render the method inaccurate.

(*c*). *By melting a known weight of copper wire under charcoal and estimating the oxygen as loss.*—Even when the heat was very slowly applied, the same spirting sometimes took place, in consequence of which the charcoal in the crucible, after the experiment, was always washed by decantation from any globules which had been projected into it. If these were weighable, they were added to the large button. The loss which the copper underwent by this treatment was estimated as oxygen. That the metal does not take up an amount of carbon sufficient to interfere materially with the results, will be shown further on. Two different samples of wire were operated upon in the following determinations of the amount of suboxide present in them, which for the sake of convenience of reference will be called A and B. A was thicker wire than B.

A, 218·24 grs. lost, by melting under charcoal, 0·76 gr., which corresponds to 3·10 per cent. of suboxide.

B, 176·48 grs. lost, by melting under charcoal, 0·635 gr., which corresponds to 3·21 per cent. of suboxide. In another experiment, 200·68 grs. lost 0·765 gr., which corresponds to 3·37 per cent. of suboxide.

According to Karsten, the amount of suboxide in tough pitch copper is under 2 per cent.; but these experiments show that it is present in larger quantity in the specimens met with in England.

Although in the preceding calculations the oxygen has always been estimated as present as suboxide, it is not maintained that the whole of it exists in that state in the copper; because commercial copper contains small quantities of various metals, generally lead, or antimony, or both, which may occur in it as oxide and not as metal. For this reason the quantity of oxygen found has always been given, as well as the amount of suboxide it corresponds to, supposing it to be combined with copper only. In the copper wire previously alluded to as A, 0·17 per cent. of lead was found by dissolving a known weight of it in nitric acid, evaporating the solution to dryness with sulphuric acid, and dissolving the anhydrous sulphates in water. The sulphate of lead was then collected on a filter, washed with water, and ignited with the usual precautions. This wire contained no antimony. Its specific gravity was 8·853. In the sample referred to as B, 0·29 per cent. of lead was found by the same treatment, and 0·31 per cent. of antimony: the antimony was separated from the copper by digesting the mixed sulphides in sulphide of ammonium containing excess of sulphur. The specific gravity of this specimen was 8·733. In a sample of sheet copper, 0·27 per cent. of lead was found, but no antimony. All of these

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split at the edges when hammered out at a dull red heat after having been melted under charcoal. They could, however, be hammered out when cold without cracking in the least. The pieces experimented on weighed about 150 or 200 grs.; possibly larger pieces would be more difficult to hammer out without cracking, but it would require an experienced coppersmith to give an opinion on the working properties of such pieces. Similar pieces as those alluded to cut from the ingot of copper made at the Hafod Works, cracked even when hammered cold after being melted in hydrogen or under charcoal. The amount of lead was not determined in this ingot. No complete analyses of copper, such as wire or sheet, were made; and in those published with which the writer is acquainted, no mention is made of oxygen as a constituent: but, what is curious, in some, metals such as potassium, calcium and magnesium are said to occur. This seems to require further investigation. Russian copper coin was found to contain oxygen, but it seems to be less essential to the malleability of this variety of copper than to the English copper; because when the oxide in Russian copper is reduced by melting under charcoal, the metal may be hammered out even at a red heat without cracking much. This seems to show greater purity, though it is not equal to electrotype copper.

Several experiments were made with the view of finding some method of melting tough pitch copper without either increasing or diminishing the amount of suboxide present in it, which, if practicable, would allow of its being melted without losing malleability. The two substances employed were common salt and chloride of calcium, the former not chemically pure chloride of sodium, but commercial salt. The method pursued was to melt the salt and drop the metal into it. It was found that electrotype copper, melted in this way, could be afterwards hammered out either cold or at a dull red heat without cracking at the edges in the least; but that ordinary copper wire, after having been so melted, cracked when hammered at a dull red heat. When working with known weights of copper wire, it was found to undergo considerable loss of weight. Thus in one experiment the wire B lost, by melting under common salt, 2·44 per cent.; in another it lost 2·05 per cent.; and in another, at a temperature just sufficient to melt it, it lost 1·35 per cent. When the salt in which the metal had been melted was afterwards dissolved in water, there was left an insoluble substance which was found to contain copper and chlorine, but which was not further examined. The loss of weight was much greater when chloride of calcium was employed, amounting in one case to 7·17 per cent. No similar experiments were made with weighed quantities of electrotype copper.

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The best method of testing copper for oxygen is to heat a strip of it, about as thick as a sixpenny-piece, in a reducing atmosphere for about half an hour, and then to try if it still retains its pliancy completely, which it will not do if it contained a notable quantity of oxygen. The diminution of specific gravity produced by the porosity arising from the reduction of the oxide will be afterwards alluded to, when treating of the effect of ammonia on red-hot copper.

Overpoled Copper.—It is generally believed that the brittleness of overpoled copper is due to the presence of carbon. In all the specimens which the writer examined, or made by melting tough pitch copper, or commercial wire or sheet in charcoal, lead or antimony, or both, were found. And it was ascertained by experiment, that the addition of these substances to pure copper in the same amount as they existed in overpoled copper, was sufficient to give to the previously pure copper the brittleness of overpoled copper. Various other substances, likely, from the process of manufacture, to occur in overpoled copper, were sought for, and experiments were made to ascertain the effect which they would have by adding them to pure copper.

Effect of Nitrogen.—This has been supposed to play a part, and it seemed likely to do so, because, according to certain statements, the presence of a most minute quantity of it in copper renders it extremely brittle (see English edition of 'Gmelin's Chemistry,' vol. v. p. 444), where the compound is described as "nitride of copper with very great excess of copper." It is said to be made by passing dry ammoniacal gas over red-hot copper wire. According to one statement, the copper increases in weight and diminishes in specific gravity. No such compound, however, seems to be formed by this method. It is true that ammonia, passed over red-hot copper wire, renders it extremely brittle; but this is evidently due to reduction of the suboxide, and not to absorption of nitrogen; because, if the gas be passed over red-hot copper wire made from electrotype copper, which contains no suboxide, the wire is found to be unaltered. Or if the commercial wire be first melted in hydrogen or under charcoal, so as to reduce the suboxide, then rolled out, and afterwards submitted to the action of ammonia at a red heat, it will be found to remain perfectly unaltered. According to the experiments made on this subject by the writer, when the wire lost its pliability by the action of ammonia at a red heat, it was always found to have lost weight and diminished in specific gravity, and water was produced. When the wire did not lose its pliability, it was found to have remained unaltered in weight and also in specific gravity, and no water was produced.

Tough pitch copper, that is, copper containing suboxide, when

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submitted to the action of ammonia, or indeed any reducing gas at a red heat, is altered in appearance on the surface, owing to the slight porosity produced. If the surface was previously polished, it becomes dull. If the experiment is continued long enough to reduce the suboxide in the centre of the piece of metal operated upon, it may afterwards be completely reduced to powder by rubbing it gently in a mortar; but if the action of the gas has not extended to the centre, a core of tough metal remains, from which the altered portion cracks off by bending the piece of metal backwards and forwards. It is curious that the copper containing suboxide is rendered more brittle by ammonia than by any other reducing gas which has been tried; but that the change arises simply from reduction of the suboxide seems proved by the fact, that copper containing no suboxide is not altered by heating in ammonia. No quantitative experiments were made to ascertain exactly whether every trace of suboxide is reduced by heating the wire in ammonia, which seems probable, because the wire, as has been said, is rendered still less pliant than when heated in hydrogen for a similar period. The specific gravity of the wire B was found to have diminished by heating in ammonia from 8·733 to 8·64.

Effect of Silicon.—Berzelius seems to have been the first to show that when a mixture of finely divided copper, silica, and charcoal is strongly heated, a combination of copper and silicon is obtained (see English edition of 'Gmelin's Chemistry,' vol. v. p. 464). It seemed, therefore, not unlikely that overpoled copper might contain some silicon, because carbonaceous and siliceous matters are in contact with it when in the furnace at the overpoled state, and that it might owe some of its properties to the silicon which it contained. It was accordingly sought for by dissolving overpoled copper in nitric acid, evaporating the solution to dryness, and heating the residue till the nitrate of copper was decomposed. The oxide, on being dissolved in hydrochloric acid, left no silica. A few experiments were, however, made to ascertain the properties of the compound more fully than seems to have been done, and to see whether it was fitted for any practical purposes. It resembles bronze in appearance, but has a pink tinge, which bronze has not. It is harder than copper, and, at least in certain proportions, tougher than bronze. One specimen, which was found by analysis to contain 1·82 per cent. of silicon, had the specific gravity of 8·70. It could be hammered and rolled out when cold, but cracked immediately when hammered at a dull red heat. It was hardened by hammering, and softened again by annealing. When "dipped" in nitric acid it became black, but could be dipped so as to retain its original colour by mixing some hydrofluoric acid with the nitric acid. A medallion

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of it was cast at Messrs. Robinson and Cotton's Works, Pimlico. It was said to require a higher temperature for casting than bronze. The surface of the casting was said to be good. No experiments were made to ascertain how much silicon copper may be made to combine with in this way; but in one specimen accidentally prepared, the quantity was very much larger than in that alluded to previously.

Effect of Carbon.—This has been supposed to be the cause of the brittleness of overpoled copper. According to Karsten, 0·05 per cent. of carbon is sufficient to cause copper to crack when hammered hot. He adds, moreover, that the presence of carbon increases the influence of small quantities of lead, antimony, and such metals as affect the malleability of copper.

A great many experiments were made to ascertain the effect of carbon upon copper. It would be useless to describe all the experiments which were made on this subject; accordingly those only which seem most conclusive will be alluded to. It is of course essential that both the copper and the carbon should be as pure as possible. In examining the result of an experiment made some years ago by Dr. Percy, the necessity of this was brought prominently forward. A quantity of finely divided copper ("best selected," from Messrs. Newton, Keats and Co.) had been diffused through charcoal powder, and heated strongly for some hours for the purpose of ascertaining the effect of carbon on copper. The shots of metal were afterwards melted together under charcoal. It was found that the metal so treated could be rolled into sheet or drawn into wire when cold, but that it cracked when worked hot. On testing this copper, it was found to contain a very notable quantity of silicon and a small quantity of phosphorus and iron. These seem to have been derived chiefly from the charcoal employed.

It was found that small pieces of electrotype copper, after having been melted under charcoal—which, as usual, had been digested in hydrochloric acid and washed with water,—could be hammered out without cracking, either when hot or cold.

A mixture of rather large pieces of electrotype copper and charcoal powder were heated together for about half an hour at a temperature approaching whiteness. The contents of the crucible were then stirred with a piece of wood, so as to cause the metal to sink to the bottom of the pot, after which it was cast into an iron ingot mould to be drawn into wire. The following somewhat similar experiment was made at the same time.

Several pieces of electrotype copper were placed in a crucible lined with charcoal powder, which was then completely filled with charcoal and exposed to a temperature approaching whiteness for about an hour. The furnace containing the crucible

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was then allowed to go out gradually, so that the metal had the opportunity of absorbing carbon at all intermediate temperatures in very favourable circumstances, being surrounded on all sides by charcoal. It was remelted under similar conditions, and the lump so obtained was sent, along with the small ingot, to Mr. David Forbes at Birmingham, who was kind enough to get them rolled into sheet and drawn into wire. No mention was made of the object of the experiment, but it was requested that they might be treated like ordinary copper, and that the men superintending the rolling mill would give an opinion on the working qualities of the metal. When the wire and sheet were returned, it was said, that although the casting was not good, yet the metal was fit for any work. It would seem from these experiments, then, that the amount of carbon in the copper did not render it brittle, and the opportunities afforded to the metal to absorb carbon were far greater than it has during the short period which elapses between the time when the copper is at the tough pitch till that at which it is at the overpoled state in the refining process; so that it is proved that the brittleness of overpoled copper is not due to carbon. A very marked effect of carbon on an ingot cast in the ordinary manner will be referred to afterwards. Whether the carbon present in overpoled copper increases the injurious effect exerted upon the malleability of the metal by the foreign metals always present, has scarcely been inquired into; because if tough pitch copper—which, as it contains oxide, cannot contain carbon—be melted in hydrogen, it becomes brittle, just as it does if melted under charcoal; and any difference in the degree of brittleness requires more experience than the writer is possessed of to detect.

A great many experiments were made for the purpose of determining whether carbon actually is present in copper which has been melted under charcoal. Of these the most conclusive will be given, from which it will be seen that the question is not yet definitely settled. Electrotype copper was melted under charcoal and afterwards rolled out. The sheet, after having been cleaned by nitric acid and boiling in solution of caustic potash, was dissolved in perchloride of iron. The solution was left at rest for a day or two so as to deposit a small quantity of suspended matter, which was washed by decantation. When dry, it had a dark bluish-gray colour. It was mixed with a little recently ignited litharge, and the mixture was heated in a small tube. Several small globules of malleable metallic lead were produced. As the substance supposed to be, or at least to contain carbon, had not been previously tested for disulphide of copper, which, had it been present, would have reduced some of the oxide of lead, this experiment alone cannot be regarded as conclusive.

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About 672 grs. of electrotype copper, which had been melted under charcoal, were mixed in the state of filings with recently ignited chromate of lead, and the mixture was heated in a combustion tube connected with a weighed set of Liebig's potash bulbs, and a combustion was made as in the case of an organic body. No gas was perceived to bubble through the potash solution. At the end of the experiment some air was drawn through the apparatus, and the potash bulbs were reweighed. They had increased only 0·115 gr. Assuming this to be carbonic acid, it corresponds to 0·031 gr. of carbon in 672 grs. of copper. This experiment is also inconclusive.

A piece of the sheet made from the ingot which had been rolled out by Mr. Forbes was cleaned by boiling in caustic potash. It weighed 221·10 grs. It was laid in a platinum basin which was immersed in a solution of sulphate of copper containing free sulphuric acid. The platinum basin was then connected with the positive pole of a galvanic battery, and a plate of copper connected with the negative pole was arranged over the basin in the solution of sulphate of copper. The whole was covered so as to exclude dust, and left till the residue of the copper in the basin was very small. It still contained a little metallic copper, which was removed by the action of a solution of perchloride of iron containing some free hydrochloric acid. The residue was then washed by decantation, dried, and weighed. It had a very dark gray, or nearly black colour, and weighed 0·08 gr. When a portion of this was heated on platinum foil it evolved a slight and peculiar odour, glowed for an instant, and left a small incombustible residue. Another portion, weighing 0·012 gr., was placed on a very small piece of platinum foil, which was introduced into a small glass tube, one end of which dipped into a solution of caustic baryta protected from the action of the air, and the other end of which was then connected with an apparatus from which a very feeble current of air, perfectly free from carbonic acid, could be sent. Some of this air was then sent through the tube containing the slip of platinum and into the solution of caustic baryta, and it was observed that no cloudiness was produced in the liquid. The heat of a spirit-lamp was then applied to that part of the tube where the piece of platinum containing the powder was placed. At first a very slight sublimate was evolved, which had the colour of sulphur, and which condensed in the cooler part of the tube. When the temperature of the platinum foil on which the powder rested had reached redness, every bubble of air as it passed through the baryta water produced a precipitate. After a short time the apparatus was disconnected, and excess of hydrochloric acid was added to the baryta solution, when the precipitate dissolved with effervescence.

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The residue upon the platinum foil was found to weigh 0·003 gr. It had a light reddish colour, and dissolved almost entirely in hydrochloric acid, the insoluble portion being probably a trace of silica which had occurred in the copper as silicon derived from the charcoal, which, although washed with hydrochloric acid and water, was yet not chemically pure. The solution contained a trace of iron, and did not give a blue colour with ammonia; but owing to the extremely small amount of it, nothing further could be detected. The only weak point in the otherwise conclusive nature of this experiment, is that the precipitate which formed in the solution of baryta might have been sulphite, and not carbonate of baryta; and this gains strength from the fact, that a slight sublimate having the colour of sulphur was observed at the beginning of the experiment. The copper which had been employed in the previous experiment was therefore examined for sulphur by dissolving it in nitro-hydrochloric acid, and afterwards boiling the solution with excess of hydrochloric acid so as to expel all nitric acid. The sulphuric acid in the solution was then precipitated, and weighed as sulphate of baryta. It was found that the copper contained 0·05 per cent. of sulphur. The very similar reaction which sulphur and carbon would have given in the preceding experiment was overlooked at the time, so that it is still an undecided question whether copper takes up any carbon by being melted under charcoal. This the writer hopes to settle soon, and will give the results in another communication. The sulphur which the copper contained might have been derived from one of three sources, but it is not certain from which, viz. from the charcoal employed, the atmosphere of the furnace, or from a little of the liquid from which the copper was precipitated by the battery still remaining in its pores. The charcoal had been boiled in hydrochloric acid and washed with water, and should therefore have contained no sulphur compounds likely to exist in charcoal; but still, as a large quantity was employed to keep the metal whilst melting from running together, it is possible that the small, though important, quantity of sulphur above mentioned may have been derived from this source.

Changes produced in the appearance and properties of copper which has been melted and cast in different ways.

When electrotype copper is melted and cooled in hydrogen, it may be hammered out without cracking, either when hot or cold. It is equally malleable after having been melted in charcoal, plate-glass, or common salt.

When melted under charcoal and allowed to cool in the crucible, the surface of the piece of metal is always found to be covered with crystalline markings, especially its upper surface,

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in the centre of which there is always a depression, owing to the contraction of the metal during solidification. In this depression are generally seen a number of crystalline points. The appearance of copper which has been melted under charcoal is influenced by a number of circumstances. One of these is deserving of special notice, because it gives rise to one of the most marked characters of overpoled copper, viz. the "rising in the mould."

When copper which has been melted under charcoal is cast into an ingot mould under ordinary circumstances, the ingot, when cooling, gives off a gas, sometimes causing projection into the air of small globules of the metal, and it solidifies with a very rough but tolerably flat surface. At other times no projection of globules takes place, and the ingot cools with a smooth surface; but in this case it is not flat; for just at the moment of solidification a quantity of still fluid metal is squeezed from the central portion of the ingot towards the centre of the upper surface, producing a ridge along it. When such ingots are fractured, they present different appearances. In the case of the former, which solidified with a rough surface, the fracture shows numerous tubular cavities which have smooth and bright metallic surfaces. The general direction of these cavities is from the sides and bottom of the ingot towards the centre of the upper surface—that part which solidified last. Many of them may be traced from the sides to the top, where they end in little craters formed by the escaping gas at the moment the ingot was becoming solid. In addition to these larger cavities there are innumerable smaller ones, which cannot be discerned without the aid of a lens, by means of which the whole substance of the metal is seen to be quite vesicular. In the case of the latter ingot, which solidified with a smooth surface and a ridge on its centre, produced by the still liquid metal being squeezed from the central portion of the ingot, the fracture is somewhat different. There are no large cavities, but the whole substance of the metal is seen to be uniformly vesicular, even by the naked eye. The effect is manifestly due to the same cause, the difference arising merely from the amount of gas evolved being greater in the one case than in the other, or else from the quicker or slower cooling of the metal.

Between the two extremes there are of course all degrees. Some ingots are minutely vesicular, and have a smooth flat surface. Others have a few larger cavities here and there among the small ones, and a smooth surface with a ridge on the centre of it. The specific gravity of a small ingot, made by melting electrotype copper under charcoal and casting under ordinary circumstances, which had this appearance, was found to be 7·851.

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Other ingots have numerous large cavities among the small ones, and a very rough surface produced by the small craters from which the gas escaped. The specific gravity of a piece of a small ingot made like the previous one, which had this appearance, was found to be 8·211; of another piece of the same, 8·285. Of course the specific gravity will vary according to the degree in which these cavities can be filled with water when taking the specific gravity. The method pursued was to place the piece of metal in boiling water and allow it to cool under water. This was found to give higher results than when the same piece of metal was placed in water under an exhausted bell-jar; but the filling of the cavities can never be perfect, because they do not communicate. The reason assigned for the well-known fact of the evolution of a gas has been, that oxygen was dissolved by the melted metal and expelled from it during solidification (see English edition of 'Gmelin's Chemistry,' vol. v. p. 402). It seems, however, certain that it cannot be oxygen, because "dry copper" and "tough pitch copper," which certainly contain oxygen, give off none during solidification: further, the surfaces of the cavities when the ingot is fractured are seen to be perfectly bright and metallic, which they would not have been had oxygen been in contact with them at the temperature at which they were formed. The gas seems to be either carbonic acid or carbonic oxide, or it may be sulphurous acid, as is shown by the following experiments.

If a piece of copper is melted under charcoal and allowed to cool in the crucible, it will be found that the appearance of the fracture, though affected by the rate of cooling and other things, never presents any trace of vesicular structure; but that if, instead of allowing it to cool in the crucible, it be cast under ordinary circumstances into an ingot mould, then the fracture does show a vesicular structure. In this case, however, another element affects the result, viz. the air through which the metal passed in being poured from the crucible into the mould, and also that in contact with its surface when in the mould. Owing to this, a portion of the copper combines with oxygen; and as this gets mixed with another portion of the copper still containing carbon or sulphur, it gives rise to the gas which causes the vesicularity. The most curious point is, that the gas is given off just at the moment of solidification, or appears to be given off only at that time. But that the evolution of the gas is due to this reaction is proved by the following fact. If copper is melted under charcoal and poured through an atmosphere of coal-gas and into an ingot mould filled with the same gas, instead of the ordinary atmosphere, the metal solidifies with a bright and smooth surface, and when fractured shows no trace of vesicular structure.

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Instead of the ridge on the surface, as there is when the air is allowed free access to the metal while pouring, there is a depression. This depression shows very markedly the crystalline structure of the metal; for during the contraction attendant on solidification, the still liquid portion seems to have been drawn into the central portion of the ingot, thus rendering evident the crystals which had formed; this is especially evident in the depression. It is not improbable that the movements of different parts of the metal during solidification, by mixing portions containing oxygen with other portions containing carbon or sulphur, may be one reason why the gas evolved is especially evident then. This difference, produced by pouring the metal into an oxidizing or reducing atmosphere, was observed very many times; and it was found quite easy, by arranging the moulds beforehand, to cast one ingot which should be vesicular and another which should not, from the same crucible,—casting the one immediately after the other in any order.

If it is desired to cast the metal which has been melted under charcoal perfectly free from cavities, the utmost precautions must be taken to exclude air. The method which was found to succeed best, was to place on the crucible a lid just large enough to cover it, having two holes in its circumference. A cover of sheet iron having two holes in it was likewise placed on the mould, which was kept full of coal-gas by passing a current of it in at one hole and allowing it to escape at the other. When the metal in the crucible was melted, it was poured into the mould through one of the holes in the lid of the crucible. The small quantity of air which entered at the other to supply its place would be instantly deoxidized by the carbon, whilst the metal was prevented from absorbing oxygen while passing from the crucible into the mould by the stream of escaping coal-gas through which it was poured, care being taken to arrange the crucible so that the metal should pass only through the gas, so as to prevent any possibility of air affecting the result. Copper which has been so cast resembles tough pitch copper in general appearance on the fractured surface. It seems to be possible to modify it slightly by the rapidity of cooling, but sufficient experiments on this point have not been made to admit of any generalizations. It may likewise be cast with a dense structure by placing in the ingot mould some fine charcoal powder, and holding the crucible as near the mould as possible, so that the metal whilst being cast is exposed to the air only for an exceedingly short time, because the instant it reaches the bottom of the mould its surface becomes covered with the charcoal powder. This method of casting copper, after fusion under charcoal in coal-gas, so as to obtain a dense structure, might perhaps be

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successfully applied to the casting of copper cylinders for calico printing, or other objects where great soundness is required.

The remarks which were made on page 420, in regard to the possibility of sulphur, and not carbon, having affected the result, apply equally to the evolution of gas described above.

The difference in the structure of copper cast dense and porous produces other effects, such as difference in colour. This depends upon the manner in which the light falls upon the fractured surface. In certain positions the colour of the fractured surface of an ingot of vesicular structure resembles that of one of dense structure, but does not show the silky lustre. When, however, the fracture of the vesicular ingot is so arranged that the light falling upon it shall enter the small cavities and be reflected from them to the observer, it then shows a fine salmon-red colour, which the fractured surface of a dense ingot shows in no light. This evidently arises from the numerous reflexions which the light undergoes in the cavities, whereby it becomes much deepened in colour.

The metal seems to be possessed of the same malleability and ductility whether dense or porous; the brittleness of overpoled copper arising, as has been stated, from impurities in the metal, and not from its vesicular structure, as was proved by getting a small porous ingot, made from electrotype copper melted under charcoal and cast in air, rolled into sheet and drawn into wire. No experiments were made to ascertain whether the wire made from a porous ingot was as great in tenacity as that made from a dense one, nor as to what was the effect of corrosive liquids upon sheet made from such ingots. A porous ingot, previous to hammering or rolling, held in a vice and struck with a hammer, breaks with ease when compared with a similar one of dense structure.

The specific gravity of electrotype copper, melted under charcoal and treated in various ways, is shown in the following Table:—

Piece of an ingot cast under ordinary circumstances, and therefore vesicular	8·535
Another piece of the same ingot	8·505
Wire before annealing, made from the same ingot	8·916
Same wire after annealing	8·919
Piece of an ingot cast in a mould containing sufficient charcoal powder to cover the surface of the metal and exclude the action of the air	8·946
Another piece of the same ingot	8·952
Piece of another ingot cast in the same manner	8·922
Wire before annealing, made from the latter ingot	8·952

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Wire before annealing, made from copper which had been melted and allowed to cool in the crucible containing charcoal	8·937
Same wire after annealing	8·930
Piece of an ingot cast in coal-gas	8·948
Another piece of the same ingot	8·958

From these experiments, it will be seen that tough pitch copper is copper containing an amount of oxygen equal to from 3 to 3·5 per cent. of suboxide of copper, in addition to small quantities of foreign metals, such as lead or antimony, or both, and that the existence of a certain amount of oxygen in it is essential to the perfect malleability of such copper.

That the brittleness of overpoled copper is not due to carbon, but to the foreign metals occurring in tough pitch copper, the influence of which upon the malleability of the metal is no longer counteracted by the oxygen compounds, owing to their having been reduced by the carbon.

That the porous structure of overpoled copper arises from a reaction between carbon or sulphur (for the anthracite employed on the large scale contains sulphur) absorbed by the metal in the furnace and oxygen absorbed by it whilst casting, which gives rise to a gas. That it may be cast with a dense structure by taking precautions to exclude the action of the air, such as by filling the mould with coal-gas, and pouring the metal so that it shall pass through coal-gas and not through air; and that this porosity is not the cause of the brittleness of overpoled copper.

That the specific gravity of copper which has been melted under charcoal and cast with a porous structure is increased by being drawn into wire, so that it is nearly as high as wire made from copper which had a dense structure at first.

That the specific gravity of copper which has been melted under charcoal and cast with a dense structure is not increased by being drawn into wire, and that the specific gravity of the wire is the same before as after annealing.

LIII. *Note on the Theory of Elliptic Motion.* By A. Cayley, *Esq.**

IF, as usual, r, θ denote the radius vector and longitude, and μ the central mass, then the Vis Viva and Force function are respectively

$$T = \tfrac{1}{2}(r'^2 + r^2\theta'^2),$$

$$U = \frac{\mu}{r}.$$

* Communicated by the Author.

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And writing

$$\frac{d\mathrm{T}}{dr'} = r' = p,$$

$$\frac{d\mathrm{T}}{d\theta'} = r^2\theta' = q,$$

we have $r' = p$, $\theta' = \frac{q}{r^2}$, and $\mathrm{T} = \frac{1}{2}\left(p^2 + \frac{q^2}{r^2}\right)$, whence putting $\mathrm{H} = \mathrm{T} - \mathrm{U}$, the value of H is

$$\mathrm{H} = \tfrac{1}{2}\left(p^2 + \frac{q^2}{r^2}\right) - \frac{\mu}{r}.$$

And by Sir W. R. Hamilton's theory, the equations of motion are

$$\frac{dr}{dt} = \frac{d\mathrm{H}}{dp}, \quad \frac{dp}{dt} = -\frac{d\mathrm{H}}{dr}$$

$$\frac{d\theta}{dt} = \frac{d\mathrm{H}}{dq}, \quad \frac{dq}{dt} = -\frac{d\mathrm{H}}{d\theta}.$$

Or substituting for H its value, the equations of motion are

$$\begin{cases} \dfrac{dr}{dt} = p, \\ \dfrac{d\theta}{dt} = \dfrac{q}{r^2}, \\ \dfrac{dp}{dt} = \dfrac{q^2}{r^3} + \dfrac{\mu}{r^2}, \\ \dfrac{dq}{dt} = 0. \end{cases}$$

Putting, as usual, $\mu = n^2a^3$, and introducing the excentric anomaly u, which is given as a function of t by means of the equation

$$nt + c = u - e\sin u$$

$\left(\text{so that } \frac{du}{dt} = \frac{n}{1 - e\cos u}\right)$, the integral equations are

$$q = na^2\sqrt{1-e^2},$$

$$p = \frac{nae\sin u}{1 - e\cos u},$$

$$r = a(1 - e\cos u),$$

$$\theta - \varpi = \tan^{-1}\left(\frac{\sqrt{1-e^2}\sin u}{\cos u - e}\right);$$

where the constants of integration a, e, c, ϖ denote as usual the mean distance, the excentricity, the mean anomaly at epoch, and the longitude of pericentre.

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Suppose that $q_0, p_0, r_0, \theta_0, u_0$ correspond to the time t_0 (q is constant, so that $q_0 = q$), and write

$$V = na^2(u - u_0 + e\sin u - e\sin u_0).$$

Joining to this the equations

$$r = a(1 - e\cos u), \quad r_0 = a(1 - e\cos u_0),$$

$$\theta - \theta_0 = \tan^{-1}\left(\frac{\sqrt{1-e^2}\sin u}{\cos u - e}\right) - \tan^{-1}\left(\frac{\sqrt{1-e^2}\sin u_0}{\cos u - e}\right),$$

u, u_0, e will be functions of $a, r, r_0, \theta, \theta_0$, and consequently ($n$ being throughout considered as a function of a) V will be a function of $a, r, r_0, \theta, \theta_0$. The function V so expressed as a function of $a, r, r_0, \theta, \theta_0$ is, in fact, the characteristic function of Sir W. R. Hamilton, and according to his theory we ought to have

$$dV = \tfrac{1}{2}n^2a(t - t_0)da + pdr + qd\theta - p_0dr_0 - q_0d\theta_0.$$

To verify this, I form the equation

$$\begin{aligned} dV = {} & \tfrac{1}{2}na(u - u_0 + e\sin u - e\sin u_0)da \\ & + na^2[(1 + e\cos u)du - (1 + e\cos u_0)du_0] \\ & + na^2(\sin u - \sin u_0)de \\ & + \frac{nae\sin u}{1 - e\cos u}\{dr - (1 - e\cos u)da - ae\sin u\,du + a\cos u\,de\} \\ & - \frac{nae\sin u_0}{1 - e\cos u_0}\{dr_0 - (1 - e\cos u_0)da - ae\sin u_0du_0 + a\cos u_0de\} \\ & + na^2\sqrt{1-e^2}\left\{d\theta - \frac{1}{\sqrt{1-e^2}(1-e\cos u)}[(1-e^2)du + \sin u\,de]\right. \\ & \left. - d\theta_0 + \frac{1}{\sqrt{1-e^2}(1-e\cos u_0)}[(1-e^2)du_0 + \sin u_0de]\right\}. \end{aligned}$$

The coefficient of du on the right-hand side is

$$\begin{aligned} & na^2(1 + e\cos u) - \frac{na^2e^2\sin^2 u}{1 - e\cos u} - \frac{na^2(1-e^2)}{1 - e\cos u} \\ & = na^2\left(1 + e\cos u - \frac{1 - e^2 + e^2\sin^2 u}{1 - e\cos u}\right), \end{aligned}$$

which vanishes, and similarly the coefficient of du_0 also vanishes: the coefficient of de is the difference of two parts, the first of which is

$$\begin{aligned} & na^2\sin u + \frac{na^2e\sin u\cos u}{1 - e\cos u} - \frac{na^2\sin u}{1 - e\cos u} \\ & = na^2\sin u\left(1 - \frac{1 - e\cos u}{1 - e\cos u}\right), \end{aligned}$$

which vanishes, and the second part in like manner also vanishes;

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the coefficient of da is the difference of two parts, the first of which is

$$\tfrac{1}{2}na(u+e\sin u)-nae\sin u=\tfrac{1}{2}na(u-e\sin u),$$

and the second is the like function of u_0; the entire coefficient therefore is $\frac{1}{2}na(u-u_0-e\sin u+e\sin u_0)$. We have therefore

$$dV=\tfrac{1}{2}na(u-u_0-e\sin u+e\sin u_0)da$$
$$+\frac{nae\sin u}{1-e\cos u}dr+na^2\sqrt{1-e^2}\,d\theta$$
$$-\frac{nae\sin u_0}{1-e\cos u_0}dr_0-na^2\sqrt{1-e^2}d\theta_0;$$

or what is the same thing,

$$dV=\tfrac{1}{2}n^2a(t-t_0)da+pdr+qd\theta-p_0dr_0-q_0d\theta_0,$$

the equation which was to be verified.

2 Stone Buildings,
March 28, 1856.

LIV. *On the Solar and Lunar Diurnal Tides of the Coasts of Ireland. By the* Rev. Samuel Haughton, *Fellow of Trinity College, Dublin.*

[Concluded from p. 272.]

Section XIII. *Mean depth of Sea deduced from the Diurnal Tide.*

SOME interesting comparisons of the solar and lunar tides may be made, deduced from the preceding results; with respect to the relative influence of the sun and moon, with respect to the tidal intervals and establishments, and with reference to the age of the lunar tide; and from each of these the mean depth of the sea may be deduced.

1. *Relative effects of the Sun and Moon.*

The following Table is formed from the constants already given:—

Relative effects of the Sun and Moon.

	Solar coefficient.	Lunar coefficient.	$\frac{S}{M}$
	ft.	ft.	
Caherciveen	0·335	0·480	0·698
Bunown	0·342	0·646	0·529
Rathmullan	0·315	0·632	0·498
Portrush	0·342	0·519	0·659
Cushendall	0·376	0·881	0·427
Donaghadee	0·383	0·868	0·441
Kingstown	0·348	0·690	0·504
Courtown	0·410	0·719	0·570
Dunmore East ...	0·192	0·441	0·436
Mean	.....	.....	0·5305

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The mean ratio of S to M here found, shows that the sun's effect on the diurnal tide is somewhat more than half the moon's effect. In Mr. Airy's discussion of the semidiurnal tides of Ireland, the following mean ratios of S to M are found*:—

Page 35	0·33	deduced from Heights.
... 106	0·34	...
... 42	0·35	deduced from Times.
... 108	0·38	...
Mean	0·35	

According to the statical theory of the tides, the ratio of the solar to the lunar effect should be the same for the diurnal and for the semidiurnal tides; but according to the dynamical theories of the tides, the coefficients in the solar and lunar tides depend in a different manner on the depth of the sea, and therefore the ratios deduced from the diurnal and semidiurnal tides should be different.

According to Mr. Airy's theory of tides with friction, the ratios of the solar to the lunar coefficients in the semidiurnal and diurnal tides are given by the following equations:—

$$\frac{\text{Diurnal solar coefficient}}{\text{Diurnal lunar coefficient}} = \frac{S}{M} \times \frac{d^3}{D^3} \times \frac{\frac{n'^2 b}{g} - 4\frac{k}{b}}{\frac{n^2 b}{g} - 4\frac{k}{b}} \quad . \quad . \quad . \quad (8)$$

$$\frac{\text{Semidiurnal solar coefficient}}{\text{Semidiurnal lunar coefficient}} = \frac{S}{M} \times \frac{d^3}{D^3} \times \frac{\frac{n'^2 b}{g} - \frac{k}{b}}{\frac{n^2 b}{g} - \frac{k}{b}} \quad . \quad . \quad . \quad (9)$$

In these equations—

S, M are the masses of the sun and moon;
d, D are the distances of the moon and sun;
n, n' are the angular velocities of the sun and moon;
k is the mean depth of the sea;
b is the mean radius of the earth;
g is the force of gravity.

Substituting the known values of all the quantities, excepting k, we find,—

$$\frac{\text{Solidiurnal coefficient}}{\text{Lunidiurnal coefficient}} = 0{\cdot}47 \frac{0{\cdot}00323 - 4\frac{k}{b}}{0{\cdot}00345 - 4\frac{k}{b}}; \quad . \quad . \quad (10)$$

* Transactions of Royal Society, 1845.

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$$\frac{\text{Semidiurnal solar coefficient}}{\text{Semidiurnal lunar coefficient}} = 0{\cdot}47\,\frac{0{\cdot}00323 - \frac{k}{b}}{0{\cdot}00345 - \frac{k}{b}}. \quad . \quad . \quad (11)$$

Substituting in equation (10) the mean value of the ratio of solar and lunar coefficients deduced from our diurnal tides, we find

$$\frac{k}{b} = \frac{1}{773}; \; k = 5{\cdot}12 \text{ miles.}$$

The value of the ratio of the semidiurnal solar to the semidiurnal lunar coefficient given by Laplace, *Mécanique Céleste*, vol. v. p. 206 (Paris, 1825), is $\frac{1}{2{\cdot}85333}$, a value derived from the famous observations made at Brest, on the Atlantic semidiurnal tides.

Substituting this value in equation (11), we find,—

$$\frac{k}{b} = \frac{1}{780}, \; k = 5{\cdot}07 \text{ miles.}$$

The agreement of these results, derived from such different data, is very remarkable, and cannot be considered as accidental.

2. *The Lunitidal and Solitidal Intervals.*

Arranging the intervals already given in a table, and reducing them to the Greenwich meridian to obtain the establishments, we find,—

Diurnal Tidal Intervals and Establishments.

	Lunitidal interval.	Solitidal interval.	Lunitidal establishment.	Solitidal establishment.
	h m	h m	h m	h m
Caherciveen	0 6	3 28	0 46	4 8
Bunown	0 81	2 52	1 11	3 32
Rathmullan	4 6	9 40	4 36	10 10
Portrush	3 43	11 30	4 9	11 56
Cushendall	7 16	11 25	7 40	11 49
Donaghadee	7 49	11 12	8 11	11 34
Kingstown	7 39	10 26	8 3	10 50
Courtown	5 28	5 1	5 53	5 26
Dunmore East ...	1 48	5 15	2 16	5 43

According to the theory of tide waves without friction, low water should occur at the time of meridian passage of the luminary, as was first pointed out by Newton, *Princip. Math.* lib. i. Prop. LXVI. cor. 18; but in consequence of friction, the phase of high water is *accelerated* by an interval equal to the difference between the tidal interval and half the period of a tidal oscilla-

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tion. We thus obtain the accelerations given in the following Table. The depths of the sea are thus found.

According to the tide theory with friction*,—

$$\frac{\text{Lunitidal acceleration}}{\text{Solitidal acceleration}} = \frac{n^2 - gkm^2}{n'^2 - gkm^2}. \quad . \quad . \quad (12)$$

In this equation,

n, n' are the angular velocities of the sun and moon,

$m = \frac{2\mu}{\lambda}$,

λ is the length of tide wave,

g is the force of gravity.

Substituting the known values of these quantities, excepting k, which is to be given in miles, we find,—

$$\frac{\text{Lunitidal acceleration}}{\text{Solitidal acceleration}} = \frac{13{\cdot}815 - k}{12{\cdot}938 - k}. \quad . \quad . \quad (13)$$

The sea depths in the following Table are calculated by means of equation (13):—

Sea depths derived from Tidal Intervals.

	Lunitidal acceleration.	Solitidal acceleration.	Ratio.	Depth of sea.
	h m	h m		miles.
Cahercivecn	12 18	8 32	1·44	10·94
Bunown	11 53	9 8	1·30	10·01
Rathmullan	8 18	2 20	3·56	12·59
Portrush	8 41	0 30	17·37	12·88
Cushendall	5 8	0 35	8·80	12·82
Donaghadee	4 35	0 48	5·73	12·75
Kingstown	4 45	1 34	3·03	12·50
Dunmore East ...	10 36	6 45	1·57	11·40
Mean	.....	.....	.....	11·986

These depths agree remarkably well together; and although they differ widely from the result obtained from heights, and from the result of Laplace's 'Brest Observations on the Semidiurnal Tide,' yet we shall find them confirmed in a remarkable manner by the depths of the sea, deducible from the age of the lunar tide.

3. *Age of the Lunar Diurnal Tide.*

Arranging the ages of the tides given in the several sections of this paper, we find,—

* Airy, 'Tides and Waves,' p. 332.

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Age of Lunar Diurnal Tide.

	High water.	Low water.
	d h	d h
Caherciveen	5 4	4 17
Bunown	4 9	4 9
Rathmullan	5 10	4 20
Portrush.....	5 9	4 19
Cushendall.....	6 19	5 3
Donaghadee	6 5	5 2
Kingstown	6 17	4 11
Courtown	6 22	3 12
Dunmore East ...	5 19	5 14

A series of values for the depth of the sea may be obtained by comparing the age of the lunar diurnal tide with the lunidiurnal acceleration of high water already given, as follows:—

By the theory of tides, including friction*, it appears that—

$$\frac{\text{Age of lunidiurnal tide}}{\text{Acceleration of lunidiurnal tide}} = \frac{i^2+gkm^2}{i^2-gkm^2} \quad . \quad (14)$$

i denoting the angular velocity of the moon, and the other letters remaining as before. Introducing the numerical values of the known quantities, we find, expressing k in miles,—

$$\frac{\text{Age of lunidiurnal tide}}{\text{Acceleration of lunidiurnal tide}} = \frac{12{\cdot}938+k}{12{\cdot}938-k} \quad . \quad (15)$$

By the aid of this equation, we find the depths of the sea, calculated in the following Table:—

Mean depth of Sea deduced from Age of Lunar Tide.

	Mean age.	Acceleration.	Ratio.	Depth of sea.
	hours.	hours.		miles.
Caherciveen	118·5	12·30	9·63	10·50
Bunown	105	11·88	8·84	10·31
Rathmullan	123	8·30	14·82	11·30
Portrush.....	122	8·68	14·05	11·22
Cushendall.....	143	5·13	27·87	12·04
Donaghadee	135·5	4·58	29·58	12·09
Kingstown	134	4·75	28·21	12·05
Dunmore East ...	136·5	10·60	12·88	11·07
Mean	.....	.....	.....	11·322

The results just obtained agree very well with each other, and with the results obtained from the diurnal solitidal and lunitidal intervals; but both results differ widely from the mean depth deduced from heights. How are we to reconcile this difference? Although this question is difficult to answer fully, yet it should

* Airy, 'Tides and Waves,' p. 333.

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be observed that probably the depth inferred from tidal intervals and ages may be the depth of the sea at a greater distance from the coast; while the depth deduced from heights is the depth of the sea after it has begun to shoal, the tide being composed partly of a derivative and partly of the original Atlantic tide.

Is it impossible, or improbable, that the result deduced from times, viz. 11·65 miles, is the depth of the central channel of the South Atlantic and of the Antarctic Oceans, while the depth deduced from heights, viz. 5·12 miles, is the mean depth of the whole Atlantic?

These and many other interesting questions suggest themselves, which I shall leave for the consideration of those concerned in the mathematical theory of the tides. The separation of the effects of the sun and moon in the diurnal tide, which has never before been made, must prove of value in correcting the bases on which the mathematical theories of the tides rest; theories which, notwithstanding the amount of mathematical genius exercised on them, must be considered as still in a most imperfect and unsatisfactory condition.

Trinity College, Dublin,
May 7, 1856.

LV. *On the Dynamical Theory of Heat.*—Part VI. *Thermo-electric Currents.* *By* William Thomson, *M.A., Professor of Natural Philosophy in the University of Glasgow.*

[Concluded from p. 388.]

§§ 156–170. *On the Thermal Effects and the Thermo-electric Excitation of Electrical Currents in Homogeneous Crystalline Solids.*

156. THE Propositions I. and II., investigated above, suggest the kind of assumptions to be made regarding the reversible thermal effects of currents in uniformly heated crystalline solids, and the electromotive forces induced by any thermal circumstances which cause inequalities of temperature in different parts. The formulæ expressing these agencies in the particular case which we have now investigated, guide us to the precise forms required to express those assumptions in the most general possible manner.

157. Let us first suppose a rectangular parallelopiped (a, b, c) of homogeneous crystalline conducting matter, completely surrounded by continuous metal of the standard thermo-electric quality touching it on all sides, to be traversed in any direction by a uniform electric current, of which the intensity components parallel to the three edges of the parallelopiped are h, i, j, and to

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be kept in all points at a uniform temperature t. Then taking ϕ, θ, ψ to denote the thermo-electric powers of bars of the substance cut from directions parallel to the edges of the parallelopiped, quantities which would be equal to one another in whatever directions those edges are if the substance were non-crystalline; and θ', θ'', ϕ', ϕ'', ψ', ψ'' other elements depending on the nature of the substance with reference to the directions of the sides of the parallelopiped, to which the name of thermo-electric obliquities may be given, and which must vanish for every system of rectangular planes through the substance if it be non-crystalline, we may assume the following expression for the reversible thermal effects of the current:—

$$\left.\begin{aligned} Q_{(b,c)} &= bc\frac{t}{J}(h\theta + i\phi'' + j\psi') \\ Q_{(c,a)} &= ca\frac{t}{J}(h\theta' + i\phi + j\psi'') \\ Q_{(a,b)} &= ab\frac{t}{J}(h\theta'' + i\phi' + j\psi) \end{aligned}\right\} \quad . \; . \; . \quad (31),$$

where $Q_{(b,c)}$, $Q_{(c,a)}$, $Q_{(a,b)}$ denote quantities of heat absorbed per second at the sides by which positive current components enter, or quantities evolved in the same time at the opposite sides. Hence if the opposite sides be kept at different temperatures, currents will pass, unless prevented by the resistance of surrounding matter; and the electromotive forces by which these currents are urged in directions parallel to the three edges of the parallelopiped have the following expressions, in which ua, vb, and wc denote the difference of temperature between corresponding points in the pairs of sides bc, ca, and ab respectively reckoned positive, when the temperature increases in the direction of positive components of current:—

$$\left.\begin{aligned} E &= -a(u\theta + v\theta' + w\theta'') \\ F &= -b(u\phi'' + v\phi + w\phi') \\ G &= -c(u\psi' + v\psi'' + w\psi) \end{aligned}\right\} \quad . \; . \; . \quad (32).$$

The negative signs are prefixed, in order that positive values of the electromotive components may correspond to forces in the direction assumed for positive components of current.

158. The most general conceivable elementary type of crystalline thermo-electric properties is expressed in the last equations, along with the equations (31) by which we arrived at them; and we shall see that every possible case of thermo-electric action in solids of whatever kind may be investigated by using them with values, and variations it may be, of the coefficients ϕ, θ, &c.,

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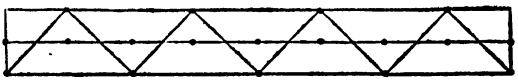
suitable to the circumstances. It might be doubted, indeed, whether these nine coefficients can be perfectly independent of one another; and indeed it might appear very probable that they are essentially reducible to six independent coefficients, from the extraordinary nature of certain conclusions which we shall show can only be obviated by supposing

$$\theta' = \phi'', \quad \theta'' = \psi', \text{ and } \phi' = \psi''.$$

Before going on to investigate any consequences from the unrestricted fundamental equations, I shall prove that it is worth while to do so, by demonstrating that a metallic structure may be actually made, which, when treated on a large scale as a continuous solid, according to the electric and thermal conditions specified for the substance with reference to which the equations (31) and (32) have been applied, shall exhibit the precise electric and thermal properties respectively expressed by those sets of equations with nine arbitrarily prescribed values for the coefficients θ, ϕ, &c.

159. Let two zigzag linear conductors of equal dimensions, each consisting of infinitely short equal lengths of infinitely fine straight wire alternately of two different metals, forming right angles at the successive junctions, be placed in perpendicular planes, and not touching one another at any point, but with a common straight line joining the points of bisection of the small



straight parts of each conductor. Let an insulating substance be moulded round them so as to form a solid bar of square section, just containing the two zigzags imbedded in it in planes parallel to its sides. Although this substance is a non-conductor of electricity, we may suppose it to have enough of conducting power for heat, or the wires of the electric conductors to be fine enough, that the conduction of heat through the bar when it is unequally heated may be sensibly the same as if its substance were homogeneous throughout, and consequently that the electric conductors take at every point the temperatures which the bar would have at the same point if they were removed. Let an infinite number of such bars, equal and similar, and of the same substance, be constructed; and let a second system of equal and similar bars be constructed with zigzag conductors of different metals from the former; and a third with other different metals; the sole condition imposed on the different zigzag conductors being that the two in each bar, and those in the bars of different systems, exercise the same resistance against electric conduction.

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Let an infinite number of bars of the first set be laid on a plane parallel to one another, with intervals between every two in order, equal to the breadth of each. Lay perpendicularly across them an infinite number of bars of the second system similarly disposed relatively to one another; place on these again bars of the first system, constituting another layer similar and parallel to the first; on this, again, a layer similar and parallel to the second; and so on till the thickness of the superimposed layers is equal to the length of each bar. Then let an infinite number of the bars of the third system be taken and pushed into the square prismatic apertures perpendicular to the plane of the layers; the cubical hollows which are left (not visible in the diagram) being previously filled up with insulating matter, such as that used in the composition of the bars. Let the complex solid cube thus formed be coated round its sides with infinitely thin connected sheets of the standard metal, so thin that the resistance to the conduction of electricity along them is infinitely great, compared to the resistance to conduction experienced by a current traversing the interior of the cube by the zigzag linear conductors imbedded in it. (For instance, we may suppose the resistance of four parallel sides of the cube to be as great as, or greater than, the resistance of each one of the zigzag linear conductors.) Let an infinite number of such cubes be built together, with their structural directions preserved parallel, so as to form a solid, which, taken on a large scale, shall be homogeneous. A rectangular parallelopiped, *abc*, of such a solid, with its sides parallel to the sides of the elementary cubes, will present exactly the thermo-electric phænomena expressed above by the equations (31) and (32), provided the thermo-electric powers ϖ_1, ϖ_1', ϖ_1'', ϖ_1''', ϖ_2, ϖ_2', ϖ_2'', ϖ_2''', and ϖ_3, ϖ_3', ϖ''_3, ϖ_3''' of the metals used in the three systems fulfil the following conditions:—

$$\left.\begin{array}{ll}
\frac{1}{4}(\varpi_1+\varpi_1'+\varpi_1''+\varpi_1''')=\theta, & \\
\frac{1}{4}(\varpi_1-\varpi_1')=\theta', & \frac{1}{4}(\varpi_1''-\varpi_1''')=\theta'', \\
\frac{1}{4}(\varpi_2+\varpi_2'+\varpi_2''+\varpi_2''')=\phi, & \\
\frac{1}{4}(\varpi_2-\varpi_2')=\phi', & \frac{1}{4}(\varpi_2''-\varpi_2''')=\phi'', \\
\frac{1}{4}(\varpi_3+\varpi_3'+\varpi_3''+\varpi_3''')=\psi, & \\
\frac{1}{4}(\varpi_3-\varpi_3')=\psi', & \frac{1}{4}(\varpi_3''-\varpi_3''')=\psi''
\end{array}\right\} \quad . \quad (33).$$

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160. To prove this, let us first consider the condition of a bar of any of the three systems, taken alone, and put in the same thermal circumstances as those in which each bar of the same system exists in the compound mass. If, for instance, we take a bar of the first system, we must suppose the temperature to vary at the rate u per unit of space along its length; at the rate v across it, perpendicularly to two of its sides; and at the rate w across it, perpendicularly to its other two sides. If l be its length, and e the breadth of each side, its ends will differ in temperature by ul; corresponding points in one pair of its sides by ve, and corresponding points in the other pair of sides by we. Now it is easily proved that the longitudinal electromotive force (that is, according to the definition, the electromotive force between conductors of the standard metal connected with its ends) would, with no difference of temperatures between its sides, and the actual difference ul between its ends, be equal to $\frac{1}{2}(\varpi_1+\varpi_1')ul$, if only the first of the zigzag conductors existed imbedded in the bar, or equal to $\frac{1}{2}(\varpi_1''+\varpi_1''')ul$, if only the second; and since the two have equal resistances to conduction, and are connected by a little square disc of the standard metal, it follows that the longitudinal electromotive force of the actual bar, with only the longitudinal variation of temperature, is

$$\tfrac{1}{4}(\varpi_1+\varpi_1'+\varpi_1''+\varpi_1''')ul.$$

Again, with only the lateral variation ve, we have in one of the zigzags a little thermo-electric battery, of a number of elements amounting to the greatest integer in $\frac{l}{2e}$, which is sensibly equal to $\frac{l}{2e}$, since the value of this is infinitely great; the electromotive force of each element is $(\varpi_1-\varpi_1')ve$; and therefore the whole electromotive force of the zigzag is

$$\frac{l}{2e}\times(\varpi-\varpi_1')ve, \text{ or } \tfrac{1}{2}l\times(\varpi_1-\varpi_1')v.$$

This battery is part of a complete circuit with the little terminal squares and the other zigzag, and therefore its electromotive force will sustain a current in one direction through itself, and in the contrary through the second zigzag; but since the resistances are equal in the two zigzags, and those of the terminal connexions may be neglected, just half the electromotive force of the first zigzag, being equal to the action and reaction between the two parts of the circuit, must remain ready to act between conductors applied to the terminal discs of the standard metal. In the circumstances now supposed, the second zigzag is throughout at one temperature, and therefore has no intrinsic electro-

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motive force; and the resultant intrinsic electromotive force of the bar is therefore

$$\tfrac{1}{4}l(\varpi_1-\varpi_1')v.$$

Similarly, if there were only the lateral variation *we* of temperature in the bar, we should find a resultant longitudinal electromotive force equal to

$$\tfrac{1}{4}l(\varpi_1''-\varpi_1''')w.$$

If all the three variations of temperature are maintained simultaneously, each will produce its own electromotive force as if the others did not exist, and the resultant electromotive force due to them all will therefore be

$$\frac{l}{4}\{(\varpi_1+\varpi_1'+\varpi_1''+\varpi_1''')u+(\varpi_1-\varpi_1')v+(\varpi_1''-\varpi_1''')w\}.$$

This being the electromotive force of each bar of the first system in any of the cubes composing the actual solid, must be the component electromotive force of each cube in the direction to which they are parallel, and therefore

$$a\tfrac{1}{4}\{(\varpi_1+\varpi_1'+\varpi_1''+\varpi_1''')u+(\varpi_1-\varpi_1')v+(\varpi_1''-\varpi_1''')w\}$$

must be the component electromotive force of the entire parallelopiped in the same direction. Similar expressions give the component electromotive forces parallel to the edges b and c of the solid, which are similarly produced by the bars of the second and third systems, and we infer the proposition which was to be proved.

161. *Cor.* By choosing metals of which the thermo-electric relations, both to the standard metal and to one another, vary, we may not only make the nine coefficients have any arbitrarily given values for a particular temperature, but we may make them each vary to any extent with a given change of temperature.

162. For the sake of convenience in comparing the actual phænomena of thermo-electric force in different directions presented by an unequally heated crystalline solid, let us now, instead of a parallelopiped imbedded in the standard metal, consider an insulated sphere of the crystalline substance, with sources of heat and cold applied at its surface, so as to maintain a uniform variation of temperature in all lines perpendicular to the parallel isothermal planes. Let the rate of variation of temperature per unit of length, perpendicular to the isothermal surfaces, be q, and let the cosines of the inclinations of this direction to the three rectangular directions in the substance to which the edges of the parallelopiped first considered were parallel, and which we shall now call the lines of reference, be l, m, n respect-

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ively. Then if we take

$$ql=u, \quad qm=v, \quad qn=w,$$

the substance of the sphere will be in exactly the same thermal condition as an equal spherical portion of the parallelopiped; and it is clear that the preceding expressions for the component electromotive forces of the parallelopiped will give the electromotive forces of the sphere between the pairs of points at the extremities of diameters coinciding with the rectangular lines of reference, if we take each of the three quantities, a, b, c, equal to the diameter of the sphere. Calling this unity, then we have

$$\left.\begin{aligned} -\mathrm{E} &= u\theta + v\theta' + w\theta'' \\ -\mathrm{F} &= u\phi'' + v\phi + w\phi' \\ -\mathrm{G} &= w\psi' + v\psi'' + w\psi \end{aligned}\right\} \quad \ldots \quad (34).$$

According to the definition given above (§ 144, Def. 3), it appears that these quantities, E, F, G, are the three components of *the intrinsic electromotive force at any point in the substance*, whether the portion of it we are considering be limited and spherical, or rectangular, or of any other shape, or be continued to any indefinite extent by homogeneous or heterogeneous solid conducting matter with any distribution of temperature through it. The component electromotive force P along a diameter of the sphere inclined to the rectangular lines of reference at angles whose cosines are l, m, n, is of course given by the equation

$$\mathrm{P} = \mathrm{E}l + \mathrm{F}m + \mathrm{G}n \quad \ldots \quad (35),$$

which may also be employed to transform the general expressions for the components of the electromotive force to any other lines of reference.

163. A question now naturally presents itself: Are there three principal axes at right angles to one another in the substance possessing properties of symmetry, with reference to the thermo-electric qualities, analogous to those which have been established for the dynamical phænomena of a solid rotating about a fixed point, and for electrostatical and for magnetic forces, in natural crystals or in substances structurally crystalline as regards electric or magnetic induction? The following transformation, suggested by Mr. Stokes's paper on the Conduction of Heat in Crystals*, in which a perfectly analogous transformation is applied to the most general conceivable equations expressing flux of heat in terms of variations of temperature along rectangular lines of reference in a solid, will show the nature of the answer.

164. The direction cosines of the line of greatest thermal variation, or the perpendicular to the isothermal planes, are

* Cambridge and Dublin Mathematical Journal, Nov. 1851.

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$\frac{u}{q}, \frac{v}{q}, \frac{w}{q}$, where q, denoting the rate of variation of temperature in the direction of that line, is given by the equation

$$q=(u^2+v^2+w^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (36).$$

Taking these values for l, m, n, in the preceding general expression for the electromotive force in any direction, we find

$$P=\frac{1}{q}\{\theta u^2+\phi v^2+\psi w^2+(\phi'+\psi'')vw+(\psi'+\theta'')wu+(\theta'+\phi'')uv\},$$

the negative sign being omitted on the understanding that P shall be considered positive when the electromotive force is from hot to cold in the substance. This formula suggests the following changes in the notation expressing the general thermo-electric coefficients:—

$$\left.\begin{array}{l} \phi'+\psi''=2\theta_1, \quad \psi'+\theta''=2\phi_1, \quad \theta'+\phi''=2\psi_1 \\ -\phi'+\psi''=2\zeta, \quad -\psi'+\theta''=2\eta, \quad -\theta'+\phi''=2\vartheta \end{array}\right\} \quad . \quad (37),$$

which reduce the general equations, and the formula itself which suggests them, to

$$\left.\begin{array}{l} -E=\theta u \ +\psi_1 v+\phi_1 w+(\eta w-\vartheta v) \\ -F=\psi_1 u+\phi v \ +\theta_1 w+(\vartheta u-\zeta w) \\ -G=\phi_1 u+\theta_1 v \ +\psi w \ +(\zeta v \ -\eta u) \end{array}\right\} \quad . \quad . \quad (38),$$

$$P=\frac{1}{q}(\theta u^2+\phi v^2+\psi w^2+2\theta_1 vw+2\phi_1 wu+2\psi_1 uv) \quad . \quad (39).$$

165. The well-known process of the reduction of the general equation of the second degree shows that three rectangular axes may be determined for which the coefficients θ_1, ϕ_1, ψ_1 in these expressions vanish, and for which, consequently, the equations become

$$\left.\begin{array}{l} -E=\theta u \ +(\eta w-\vartheta v) \\ -F=\phi v \ +(\vartheta u-\zeta w) \\ -G=\psi w+(\zeta v \ -\eta u) \end{array}\right\} \quad . \quad . \quad . \quad (40),$$

$$P=\frac{1}{q}(\theta u^2+\phi v^2+\psi w^2) \quad . \quad . \quad . \quad . \quad (41).$$

166. The law of transformation of the binomial terms $(\eta w-\vartheta v)$, &c. in these expressions is clearly, that if ρ denote a quantity independent of the lines of reference, and expressing a specific thermo-electric quality of the substance, which I shall call its thermo-electric rotatory power, and if λ, μ, ν denote the inclinations of a certain axis fixed in the substance, which I shall call its axis of thermo-electric rotation, to any three rectangular lines

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of reference, then the values of ζ, η, ϑ for these lines of reference are as follows:—

$$\zeta=\rho\cos\lambda,\quad \eta=\rho\cos\mu,\quad \vartheta=\rho\cos\nu.$$

If i denote the inclination of the direction $\left(\frac{u}{q},\frac{v}{q},\frac{w}{q}\right)$, in which the temperature varies most rapidly, to the axis of thermo-electric rotation, and if α, β, γ denote the angles at which a line perpendicular to the plane of this angle i is inclined to the axes of reference, we have

$$\left.\begin{aligned}\eta w-\vartheta v&=\rho q\sin i\cos\alpha\\ \vartheta u-\zeta w&=\rho q\sin i\cos\beta\\ \zeta v-\eta u&=\rho q\sin i\cos\gamma\end{aligned}\right\}\quad\ldots\quad(42).$$

Hence we see that the last terms of the general formula for the component electromotive forces along the lines of reference express the components of an electromotive force acting along a line perpendicular both to the axis of thermo-electric rotation, and to the direct line from hot to cold in the substance, and equal in magnitude to the greatest rate of variation of temperature perpendicular to that axis, multiplied by the coefficient ρ.

167. Or again, if we consider a uniform circular ring of rectangular section, cut from any plane of the substance inclined at an angle λ to a plane perpendicular to the axis of thermo-electric rotation, and if the temperature of the outer and inner cylindrical surfaces of this ring be kept each uniform, but different from one another, so that there may be a constant rate of variation, q, of temperature in the radial direction, but no variation either tangentially or in the transverse direction perpendicular to the plane of the ring, we find immediately, from (42), that the last terms of the general expressions indicate a tangential electromotive force, equal in value to $\rho q\cos\lambda$, acting uniformly all round the ring. This tangential force vanishes if the plane of the ring contain the axis of thermo-electric rotation, and is greatest when the ring is in a plane perpendicular to the same axis.

168. The peculiar quality of a solid expressed by these terms would be destroyed by cutting it into an infinite number of plates of equal infinitely small thickness, inverting every second plate, and putting them all together again into a continuous solid, in planes perpendicular to the axis of thermo-electric rotation; a process which would clearly not in any way affect the thermo-electric relations expressed by the first term of the general expressions for the components of electromotive force; and it is therefore of a type, to which also belongs the rotatory property with reference to light discovered by Faraday as induced by magnetization in transparent solids, which I shall call dipolar, to distin-

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guish it from such a rotatory property with reference to light as that which is naturally possessed by many transparent liquids and solids, and which may be called an isotropic rotatory property. The axis of thermo-electric rotation, since the agency distinguishing it as a line also distinguishes between the two directions in it, may be called a dipolar axis; so may the axis of rotation of a rotating rigid body*, or the direction of magnetization of a magnetized element of matter; and its general type is obviously different from that of a principal axis of inertia of a rigid body, or a principal axis of magnetic inductive capacity in a crystal, or a line of mechanical tension in a solid; any of which may be called an isotropic axis.

169. The general directional properties expressed by the first terms of the second members of (40) are perfectly symmetrical regarding the three rectangular lines of reference, and are of a type so familiar that they require no explanation here. We conclude that every substance has three principal isotropic axes of maximum and minimum properties regarding thermo-electric power, which are at right angles to one another; but that it is only for a particular class of conceivable substances that the thermo-electric properties are entirely symmetrical with reference to these axes; all substances for which the rotatory power, ρ, does not vanish, having besides a dipolar axis of thermo-electric rotation which may be inclined in any way to them.

170. These principal isotropic axes lose distinction from all other directions in the solid when the thermo-electric powers along them (the values of the coefficients θ, ϕ, ψ) are equal; but a rotatory property, distinguishing a certain line as a dipolar axis, may still exist. By § 159, we see how metallic structures possessing any of these properties (for instance, having equal thermo-electric power in all directions, and possessing a given rotatory power, ρ, in a given direction about a given system of parallel lines) may be actually made.

171. [*Added, July* 1854.] It is far from improbable that a piece of iron in a state of magnetization, which I have, since § 147 was written, ascertained to possess different thermo-electric properties in different directions, may also possess rotatory thermo-electric power†, distinguishing its axis of magnetization,

* [*Added, Liverpool, Sept.* 27, 1854.]—As is perfectly illustrated by M. Foucault's beautiful experiment of a rotating solid, placing its axis parallel to that of the earth's, and so turned that it may itself be rotating in the same direction as the earth; which the meeting of the British Association just concluded has given me an opportunity of witnessing.

† [*Added, Sept.* 13, 1854.]—By an experiment made to test its existence, which has given only negative results, I have ascertained that this "rotatory power," if it exists in inductively magnetized iron at all, must be very

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which is essentially, in its magnetic character, dipolar, as thermo-electrically dipolar also.

§§ 172–181. *On the general equations of Thermo-electric Action in any homogeneous or heterogeneous crystallized or non-crystallized solid.*

172. Let t denote the absolute temperature at any point, x, y, z, of a solid. Let $\theta, \phi, \psi, \theta', \phi', \psi', \theta'', \phi'', \psi''$ be the values of the nine thermo-electric coefficients for the substance at this point, quantities which may vary from point to point, either by heterogeneousness of the solid, or in virtue of non-uniformity of its temperature. Let h, i, j be the components of the intensity of electric current through the same point (x, y, z).

173. Then, applying equations (31) of § 157 to infinitely small, contiguous, rectangular parallelopipeds in the neighbourhood of the point (x, y, z), and denoting by H $dx\,dy\,dz$ the resultant reversible absorption of heat occasioned by the electric current across the infinitely small element $dx\,dy\,dz$, we find

$$\mathrm{H}=\frac{t}{\mathrm{J}}\left\{\frac{d}{dx}(h\theta+i\phi''+j\psi')+\frac{d}{dy}(h\theta'+i\phi+j\psi'')+\frac{d}{dz}(h\theta''+i\phi'+j\psi)\right\} \quad (43).$$

174. By the analysis of discontinuous functions, this expression may be applied not only to homogeneous or to continuously varying heterogeneous substances, but to abrupt transitions from one kind of substance to another. Still it may be convenient to have formulæ immediately applicable to such cases, and therefore I add the following expression for the reversible thermal effect in any part of the bounding surface separating the given solid from a solid of the standard metal in contact with it:—

$$\mathrm{Q}=\frac{t}{\mathrm{J}}\{p(h\theta+i\phi''+j\psi')+q(h\theta'+i\phi+j\psi'')+r(h\theta''+i\phi'+j\psi)\}. \quad (44),$$

where Q denotes the quantity of heat absorbed per second per unit of surface at a point of the bounding surface, and (p, q, r) the direction cosines of a normal to the surface at the same point.

175. Equations (34) give explicitly the intrinsic electromotive force at any point of the solid when the distribution of temperature is given; but we must take into account also the reaction proceeding from the surrounding matter, to get the efficient electromotive force determining the current through any part of the body. This reaction will be the electrostatical resultant force due to accumulations of electricity at the bounding surface and in the interior of the conducting mass throughout which the

small in comparison with the amount by which the thermo-electric power in the direction of magnetization differs from the thermo-electric power of the same metal not magnetized.

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electrical circuits are completed. Hence if V denote the electrical potential at (x, y, z) due to these accumulations, the components of the reactional electromotive force are

$$-\frac{dV}{dx}, \quad -\frac{dV}{dy}, \quad -\frac{dV}{dz};$$

and the components of the efficient electromotive force in the solid are therefore

$$E-\frac{dV}{dx}, \quad F-\frac{dV}{dy}, \quad G-\frac{dV}{dz},$$

where E, F, G are given by the following equations, derived from (34) by substituting for u, v, w their values $\frac{dt}{dx}, \frac{dt}{dy}, \frac{dt}{dz}$, in terms of the notation now introduced:—

$$\left.\begin{aligned} -E&=\frac{dt}{dx}\theta+\frac{dt}{dy}\theta'+\frac{dt}{dz}\theta'' \\ -F&=\frac{dt}{dx}\phi''+\frac{dt}{dy}\phi+\frac{dt}{dz}\phi' \\ -G&=\frac{dt}{dx}\psi'+\frac{dt}{dy}\psi''+\frac{dt}{dz}\psi \end{aligned}\right\} \quad . \quad . \quad . \quad (45).$$

176. The body, being crystalline, probably possesses different electrical conductivities in different directions, and the relation between current and electromotive force cannot, without hypothesis, be expressed with less than nine coefficients. These, which we shall call the coefficients of electric conductivity, we shall denote by κ, λ, &c.; and we have the following equations, expressing by means of them the components of the intensity of electric current in terms of the efficient electromotive force at any point of the solid:—

$$\left.\begin{aligned} h&=\kappa\left(E-\frac{dV}{dx}\right)+\kappa'\left(F-\frac{dV}{dy}\right)+\kappa''\left(G-\frac{dV}{dz}\right) \\ i&=\lambda''\left(E-\frac{dV}{dx}\right)+\lambda\left(F-\frac{dV}{dy}\right)+\lambda'\left(G-\frac{dV}{dz}\right) \\ j&=\mu'\left(E-\frac{dV}{dx}\right)+\mu''\left(F-\frac{dV}{dy}\right)+\mu\left(G-\frac{dV}{dz}\right) \end{aligned}\right\} \quad . \quad (46).$$

These equations (45) and (46), with

$$\frac{dh}{dx}+\frac{di}{dy}+\frac{dj}{dz}=0 \quad . \quad . \quad . \quad . \quad . \quad (47),$$

which expresses that as much electricity flows out of any portion of the solid as into it, in any time, (in all seven equations,) are sufficient to determine the seven functions E, F, G, V, h, i, j,

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for every point of the solid, subject to whatever conditions may be prescribed for the bounding surface, and so to complete the problem of finding the motion of electricity across the body in its actual circumstances; provided the values of $\frac{dt}{dx}, \frac{dt}{dy}, \frac{dt}{dz}$ are known, as they will be when the distribution of temperature is given. We may certainly, in an electrical problem such as this, suppose the temperature actually given at every point of the solid considered, since we may conceive thermal sources distributed through its interior to make the temperature have an arbitrary value at every point.

177. Yet practically the temperature will, in all ordinary cases, follow by conduction from given thermal circumstances at the surface. The equations of motion of heat, by which, along with those of thermo-electric force, such problems may be solved, are as follows:—(1), three equations,

$$\left.\begin{aligned} \zeta &= -\left(k\frac{dt}{dx} + k'\frac{dt}{dy} + k''\frac{dt}{dz}\right) \\ \eta &= -\left(l''\frac{dt}{dx} + l\frac{dt}{dy} + l'\frac{dt}{dz}\right) \\ \vartheta &= -\left(m'\frac{dt}{dx} + m''\frac{dt}{dy} + m\frac{dt}{dz}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (48)$$

to express the components ζ, η, ϑ of the "flux of heat" at any point of the solid, in terms of the variations of temperature $\left(\frac{dt}{dx}, \frac{dt}{dy}, \frac{dt}{dz}\right)$ multiplied by coefficients k, l, m, k', &c., which may be called the nine coefficients of thermal conductivity of the substance; and (2), the single equation

$$\frac{d\zeta}{dx} + \frac{d\eta}{dy} + \frac{d\vartheta}{dz} =$$

$$-\frac{t}{\mathrm{J}}\left\{\frac{d}{dx}(h\theta + i\phi'' + j\psi') + \frac{d}{dy}(h\theta' + i\phi + j\psi'') + \frac{d}{dz}(h\theta'' + i\phi' + j\psi)\right\}$$

$$+\frac{1}{\mathrm{J}}\left\{h\left(\mathrm{E} - \frac{d\mathrm{V}}{dx}\right) + i\left(\mathrm{F} - \frac{d\mathrm{V}}{dy}\right) + j\left(\mathrm{G} - \frac{d\mathrm{V}}{dz}\right)\right\} \quad . \quad . \quad (49),$$

of which the first member expresses the rate at which heat flows out of any part of the solid per unit of volume; and the second member, to which it is equated, the resultant thermal agency (positive when there is, on the whole, evolution at xyz) produced by the electric currents.

178. The general treatment of these eleven equations, (45), (46), (47), (48), (49), leads to two non-linear partial differential

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equations of the second order and degree for the determination of the functions t and V.

179. It may be remarked, however, that the second term of the second member of (49), when the prefixed negative sign is removed, expresses the frictional generation of heat by currents through the solid, and will therefore, when the electromotive forces in action are solely thermo-electric, be very small, even in comparison with the reversible generation and absorption of heat in various parts of the body, provided the differences of temperature between these different localities are small fractions of the temperature, on the absolute scale from its zero. Excepting, then, cases in which there are wide ranges (for instance, of 50° C. or more) of temperature, the second principal term of the second member of (49) may be neglected, and the partial differential equations to which t and V are subject will become linear; so that one of the unknown functions may be readily eliminated, and a linear equation of the fourth order obtained for the determination of the other.

180. Further, it may be remarked that probably in most, if not in all known cases, the reversible as well as the frictional thermal action of the currents, when excited by thermo-electric force alone, is very small in comparison with that of conduction, perhaps quite insensible. [See above, § 106.] Hence, except when more powerful electromotive forces than the thermo-electric forces of the solid itself, and of its relation to the matter touching it round its surface, act to drive currents through it, we may possibly in all, certainly in many cases, neglect the entire second member of (49) without sensible loss of accuracy; and we then have a differential equation of the second order for the determination of the temperature in the interior of the body, simply from ordinary conduction, according to the conditions imposed on its surface. To express these last conditions generally, a superficial application of the three equations (48) with their nine independent coefficients is required.

181. When t is either given or determined in any way, the solution of the purely electrical problem is, as was remarked above, to be had from the seven equations (45), (46), and (47). These lead to a single partial differential equation of the second order for the determination of V through the interior, subject to conditions as to electromotive force and electrical currents across the surface, for the expression of which superficial applications of (45) and (46) will be required. When V is determined, the solution of the problem is given by (45) and (46), expressing respectively the electromotive force and the motion of electricity through the solid.

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LVI. *On the Discovery of the true form of* Carnot's *Function.* *By* Professor William Thomson.

To the Editors of the Philosophical Magazine and Journal.

Oakfield, Moss Side, Manchester,
May 12, 1856.

Gentlemen,

M. CLAUSIUS, in a letter of date March 20, 1856, addressed to yourselves, and published this month in your Magazine, objects to a statement he supposes me to have made in 1851, and to have frequently repeated since that time, that Mr. Joule had discovered "the theorem, that Carnot's function $\left(\mathrm{C} \text{ or } \frac{1}{\mu}\right)$ 'is nothing more than the absolute temperature multiplied by the equivalent of heat for the unit of work.'" He attributes the discovery of the true form of Carnot's function to Holtzmann, who gave the formula referred to in a paper which appeared as early as 1845; but he believes that in his own paper "On the Moving Force of Heat," communicated to the Berlin Academy in 1850, the principles upon which that formula is based were first correctly explained.

Allow me to answer the charge he makes against me by quoting what I said with reference to the "discovery" for which M. Clausius claims priority.

"This formula was suggested to me by Mr. Joule, in a letter dated December 9, 1848, as probably a true expression for μ, being required to reconcile the expression derived from Carnot's theory (which I had communicated to him) for the heat evolved in terms of the work spent in the compression of a gas, with the hypothesis that the latter of these is exactly the mechanical equivalent of the former, which he had adopted in consequence of its being, at least approximately, verified by his own experiments. This, which will be called Mayer's hypothesis, from its having been first assumed by Mayer, is also assumed by Clausius without any reason from experiment; and an expression for μ, the same as the preceding, is consequently adopted by him as the foundation of his mathematical deductions from elementary reasoning regarding the motive power of heat*."

This passage is the sequel to the extract quoted by M. Clausius in his letter to you, and appeared in the same Part of the 'Transactions,' and in the same volume of the Philosophical Magazine. When it is read, I think it will be admitted that I did not do injustice to his claims in writing the following sentence two years

* "On a Method of discovering experimentally the Relation between the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid." (Trans. Roy. Soc. Edinb. April 17, 1851; or Phil. Mag. December 1852.)

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later, of which the words "Mr. Joule's conjecture" have called forth his reclamation:—

"A more convenient assumption has since been pointed to by Mr. Joule's conjecture, that Carnot's function is equal to the mechanical equivalent of the thermal unit divided by the temperature by the air thermometer from its zero of expansion; an assumption which experiments on the thermal effects of air escaping through a porous plug, undertaken by him in conjunction with myself for the purpose of testing it (Phil. Mag. October 1852), have shown to be not rigorously, but very approximatively true."

I rémain, Gentlemen,
Yours very faithfully,
WILLIAM THOMSON.

LVII. *On a new Method of observing the Spectra of Stars.* By WILLIAM SWAN, *F.R.S.E.**

ABOUT the beginning of March last, being engaged in experiments which involved observations of the dark lines in the solar spectrum, I attempted to observe the spectrum of Sirius; but the light of the star, enfeebled by two reflexions at the specula of a heliostat which formed part of my apparatus, and accompanied by a blaze of gas-light from the street lamps, was too faint to be visible. The failure of this experiment was so obviously due to the unfavourable nature of the trial, that I determined to renew it on the first opportunity.

Although I was fully aware that Fraunhofer had observed dark lines in the spectra of several stars, I did not suppose that he had executed exact measurements, as no reference to such observations is made in his celebrated paper on the dark lines in the solar spectrum, where he describes the appearance of several stellar spectra†. I therefore resolved, as soon as I had leisure, to attempt a series of observations on the spectra of stars; and the recollection of a method of ascertaining refractive indices, described by me in 1844, immediately suggested an arrangement by which accurate results could be readily obtained‡.

I have, however, more recently found that Fraunhofer actually measured the deviations of the rays in star-light; and before describing my own, I will shortly explain his mode of observation§.

* Communicated by the Author.

† Schumacher's *Astronomische Abhandlungen*, 1823.

‡ Edinb. New Phil. Journ., Jan. 1844.

§ See his paper translated in Edinburgh Journal of Science, vol. viii. p. 7, 1828.

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A prism was mounted before the object-glass of a telescope furnished with a micrometer; and to this telescope a smaller one was attached, at such an angle, that one observer could view a star by direct vision, while another simultaneously observed its refracted image. The first observer made the star intersect the wires of the small telescope; the second brought the micrometer wire to intersect a line of the spectrum. The inclination of the optical axes of the telescopes being known, the deviation of the refracted rays was then readily found.

The difficulty of measuring the deviation of the refracted light of a star, which arises from the apparent diurnal motion of the heavenly bodies, is completely overcome by this mode of observation, but otherwise it is very inconvenient. Other methods of observation would be to refract the light in a vertical plane, and to observe so near the meridian that the star's variation in altitude would be insensible; or to observe with an equatoreal telescope, refracting the light in the plane of the declination circle: but the first of these devices is obviously all but impracticable, and the second depends on difficult instrumental adjustments, while both involve corrections for atmospherical refraction. The method I have now to propose is perfectly free from all these objections.

A prism is placed on a stage, furnished with proper adjusting screws, immediately behind the horizon-glass of a sextant or reflecting circle, and in the prolongation of the axis of the telescope through which the star is to be observed; the whole being mounted on a suitable stand. The observer can thus see, at once, both the image of the star formed by rays which have been reflected at the mirrors of the sextant or reflecting circle, and the spectrum formed by rays which have passed through the prism, which has been adjusted to its position of minimum deviation. The image of the star being then made to coincide with any of the lines in its spectrum, the deviation of the rays is obtained directly, by reading off the angle indicated by the sextant or circle.

I have had an apparatus constructed on this principle, but owing to unfavourable weather, I have as yet been able to make only one imperfect observation. This was on Mars, on the 16th of May. Notwithstanding the proximity of the planet to the moon, and the brightness of that luminary, then twelve days old, the spectrum was more brilliant than I anticipated. The sharp disc of the planet's reflected image, as the tangent-screw was turned, glided on the edge of the spectrum like a bead along a thread; and contact could be made with the utmost nicety, the brightness of the reflected image being reduced to suit that of the spectrum.

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It is perhaps unnecessary to point out the advantages of this mode of observation. These are the complete elimination of the effects of the star's diurnal motion, and of refraction, advantages which it shares in common with Fraunhofer's method. It possesses, moreover, the additional recommendations of requiring only one observer, and of dispensing with the necessity of illuminating either the field or the wires of the telescope. Those who have had any experience of the difficulty attending observations of faint spectra will appreciate the value of this last property.

4 Duke Street, Edinburgh,
May 17, 1856.

LVIII. *A Trifle on Projectiles.* By J. J. Sylvester, *Professor of Mathematics at the Royal Military Academy**.

IN teaching the subject of projectiles *in vacuo*, the following solution has presented itself to me of a question not wholly without practical interest, viz. of determining the angle of projection to give the best range in the most general case, viz. when a gun is fired upon a slope at a given vertical height above the slope. The solution is not wholly either without theoretical interest in point of method, as leading to a result of some little complexity in maxima and minima by very simple calculations, and without the aid of the differential calculus. Therefore I venture to submit it to the readers of the Philosophical Magazine. In the next Number of the Magazine I hope to have leisure to lay before them a subject of much greater interest, also belonging to the theory of projectiles, showing how, by the oblique action of gravity combined with the earth's rotation, a pendulum suitably adjusted may be caused to advance in a westerly direction, and so the earth be made the means of impelling a light carriage without any visible motive force, or any influence of magnetism.

To this pendulum I give, for reasons which will be apparent when the matter is more clearly set forth, and in contradistinction to the ordinary fixed or circular pendulum on the one hand, and to Foucault's free or spherical pendulum on the other, the name of the *Cylindrical* or *Travelling* Pendulum. But to resume the business of this present communication: let us begin with determining the angle of projection to give the maximum range when a gun is fired from a point *in* a plane sloping at an angle i from the horizon.

This question is most simply solved (the result itself is of course familiar to all who will read this paper) by resolving the

* Communicated by the Author.

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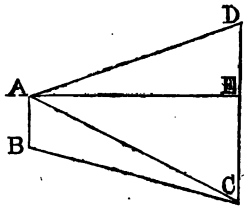
velocity V, supposed to make an angle θ with the horizon, as also g, the accelerating force of gravity, each into two parts, V into $V\cos(\theta+i)$ and $V\sin(\theta+i)$, and g into $g\sin i$ and $g\cos i$, respectively parallel and perpendicular to the plane of the slope.

The time of flight is of course found by looking to the perpendicular part of the velocity and of gravity alone, and is evidently $2\frac{V\sin(\theta+i)}{g\cos i}$, which call τ; the range will evidently be

$$\frac{V\cos\theta\,.\,\tau}{\cos i},\ i.e.\ \frac{V^2}{g\cos i}\big(\sin(2\theta+i)+\sin i\big).$$

Hence the best angle of range for this case is found by making $2\theta+i=90$, $\theta=\frac{1}{2}(90-i)$.

Now let us proceed to apply this result to the general case, as in the figure below, where BC is the slope upon which the range is to be measured, A the point of projection, AD the direction which gives the maximum range upon the slope, and BC the actual extent of this range; then I say AD is the direction which would give also the best range upon the slope AC. Since if, with the given velocity of projection, any other direction than AD would give a better range upon AC, the path corresponding to such direction must evidently cut BC at a point beyond C in that line in order to strike a point beyond C in the line AC.



Hence if we draw the horizontal line AE, we know by the preceding case that the angle $DAE=\frac{1}{2}CAB$*.

Let $CAB=\phi$, which is to be found; also let $AB=h$, and the inclination of BC to $AD=\iota$, h and ι being given; and let $t=$ time of flight, then

$$CAD=(90^\circ-\phi)+\frac{\phi}{2}$$

$$\left(=90^\circ-\frac{\phi}{2}\right).$$

* This equation, and the isoscelism of the principal triangle of the figure to which it leads, would not readily present themselves to notice in the direct method of seeking the maximum range. It is for the sake of this pleasing geometrical relation, not unmixed perhaps with a desire of exhibiting the simple yet delicate turn of reasoning, the agreeable little point of method (a fly embalmed in amber) contained in the immediately preceding paragraph, that I have thought this trifle worth preserving in the pages of the Magazine.

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Hence also

$$\mathrm{ADC}=180-\phi-\left(90°-\frac{\phi}{2}\right)=90°-\frac{\phi}{2}.$$

Hence

$$\frac{1}{2}gt^2=\mathrm{CD}=\mathrm{AC}=h\frac{\sin \mathrm{ABC}}{\sin \mathrm{ACB}}$$
$$=\frac{h\cos\iota}{\cos(\iota+\phi)};$$

and

$$v\cos\frac{\phi}{2}t=\mathrm{AD}=\frac{h\sin\phi\cos\iota}{\cos(\iota+\phi)}.$$

Hence eliminating t, we have

$$\frac{v^2}{gh\cos\iota}=\frac{(\sin\phi)^2}{(1+\cos\phi)}\cdot\frac{1}{\cos(\iota+\phi)}=\frac{1-\cos\phi}{\cos(\iota+\phi)}.$$

If $\iota=0$, *i. e.* if the gun is fired from the top of a battery commanding a level plain, we have simply

$$\sec\phi=1+\frac{v^2}{gh},$$

which gives ϕ the double of the angle of elevation.

In other cases we may make $\phi+\iota=\psi$, we have then

$$\frac{1-\cos(\psi-\iota)}{\cos\psi}=\frac{1}{\cos\psi}-\frac{\sin\psi}{\cos\psi}\sin\iota-\cos\iota=\frac{v^2}{gh}\cos\iota.$$

Let

$$\left(1+\frac{v^2}{gh}\right)\cot\iota=\cot\epsilon;$$

then

$$\frac{\sin\iota}{\sin\epsilon}\cos(\psi-\epsilon)=1,$$
$$\cos(\psi-\epsilon)=\frac{\sin\epsilon}{\sin\iota}.$$

or

$$\cos(\phi+\iota-\epsilon)=\frac{\sin\epsilon}{\sin\iota},$$

from which ϕ, the double of the angle of elevation, may be determined.

Calling $\frac{\sin\epsilon}{\sin\iota}=\cos\mu$, and taking ϕ_1, ϕ_2 as the two values of ϕ, we have

$$2\phi_1+\iota-\epsilon=\mu,$$
$$2\phi_2+\iota-\epsilon=360-\mu.$$

ϕ_1, ϕ_2 correspond to the angles of projection *down and up* the slope respectively, the one affording what in an *algebraical* sense is a maximum, and the other a minimum, but of course, *arith-*

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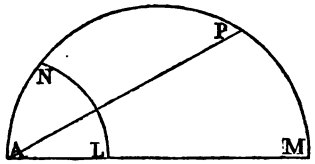
metically speaking, both giving maximum values of the range; the angle between them is $180-\mu$.

Thus when $h=0$, so that $\sin\epsilon=0$, $\mu=90$, and $\phi_1-\phi_2$ is a right angle, as may easily be verified.

It may be worth while to exhibit the geometrical construction for the case of firing from a gun in position commanding a *horizontal plane.*



Let A be the position of the gun, LN a portion of a circle to radius AL which represents the height of the gun above the plain, LM twice the height due to the velocity of projection, ANM a semicircle on AM, P the point in it bisecting the arc MN, then (abstraction made of the resistance of the air) AP is the elevation at which the gun must be pointed to give the greatest range on the plain below, for sec 2PAM obviously $=1+\frac{(\text{velocity of ball})^2}{g\,.\,\text{AL}}$.

Suppose a sea battery as much as 300 feet above the water, and a cannon-ball projected at the low rate of 1200 feet per second (which is less than that of a common musket-ball), we should have twice the height due to the velocity of projection equal to 44720, and therefore

$$\sec 2\alpha=\frac{44720}{1200}+1$$
$$=38{,}2666,$$

and consequently $2\alpha=88^\circ\ 30'\ 9''$

or $\alpha=44^\circ\ 15'\ 5''$,

differing very little from 45°; showing that certainly in a non-resisting medium, and in all probability in air, the height of the point of fire above the plane which it commands will very little indeed influence, under any conceivable circumstances of practice, the angle of elevation which gives the best range.

U, The Common, Woolwich,
April 30, 1856.

LIX. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D.*

[Continued from p. 378.]

In the January Number of Liebig's *Annalen*, Dean and Wöhler communicate the results of researches on tellurium and selenium compounds. By acting on sulphamylate of potash by

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telluride of potassium, a reddish-yellow oily body is obtained. The mode of its formation leads to the supposition that it is *telkuramyle*, $C^{10}H^{11}Te$, but the analyses gave a result which agrees more closely with the composition of *tellurbutyle*, C^8H^9Te. It is evidently an impure substance. It seems to act as a radical, and forms compounds with chlorine, iodine, bromine, and nitric acid, of which the latter alone is crystallizable. The oxide, obtained by digesting the chlorine compound with oxide of silver and water, is soluble in water, and is so strong an alkali that it liberates ammonia from chloride of ammonium.

By acting on selenide of potassium with sulphomethylate of baryta, selenmethyle, $C^2H^3S^2$, is formed. It is a reddish-yellow, very mobile liquid, of an extremely unpleasant odour. It is heavier than, and insoluble in water. It has more similarity in its reactions with sulphide of æthyle than with selenæthyle. When acted on by nitric acid at a gentle heat, methyloselenious acid, $HO + C^2H^3O, 2SeO^2$, is produced. This acid crystallizes in groups of colourless prisms, and forms well-defined crystalline salts with ammonia, baryta, and silver, in which the water of the acid is substituted by one equivalent of the base. Heated with hydrochloric acid, chloromethyloselenious acid is formed. This has the formula $HO + C^2H^3Cl, 2SeO^2$. It is readily obtained in crystals. Similar compounds with bromine and iodine were produced.

M. Stölzel analysed several specimens of green and of blue ultramarine, and tried the action of various chemical reagents on them. The methods used in the analysis are also described. The results he arrived at may be given in his own summary of them.

Blue ultramarine exhibited, under exclusion of the air, various degrees of resistance to fire; at a higher temperature it lost its colour, a mass being left behind which developed sulphurous acid on the addition of hydrochloric acid: blue ultramarine, prepared by igniting green, remained unchanged, and liberated sulphuretted hydrogen on the addition of hydrochloric acid. Air, oxygen, chlorate of potash, saltpetre, sulphurous acid, and hydrogen decompose the colour of both ultramarines at a high temperature, solid potash at a moderate heat, and strong acids and chlorine in the cold.

When hydrogen is passed over blue ultramarine heated, sulphuretted hydrogen is evolved; this is not the case with green ultramarine. Both leave, after this treatment, a grayish mass, which in the oxidizing flame of the blowpipe becomes first green and then blue.

Solid potash and soda, and still more perceptibly potassium

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and sodium, converted both ultramarines when gently heated into red ultramarine. Green ultramarines, when not acted upon by strong agents, had always a tendency to pass into blue.

The same subject has been ably investigated by Breunlin, an abridgment of whose paper will appear in next month's Magazine.

With a view of obtaining an easy method for the preparation of fluoride of sodium, which might serve as a useful material for the preparation of fluorine compounds in general, Briegleb investigated the action of phosphate of soda on fluor-spar. The two substances, mixed together in the proper proportions, were fused in a Hessian crucible, and the fused mass poured on an iron plate. The last parts poured out of the crucible were composed of a mass of small crystals. These were insoluble in water, and on examination were found to be apatite. This formation of apatite has been already observed by Manross. The fused mass was boiled with water for some time; some fluoride of sodium was obtained, but it bore no adequate proportion to the quantity that the theory required. An attempt to produce fluoride of potassium in this way gave still less favourable results.

When, instead of extracting the fused mass with boiling water, it was digested with water on the water-bath for some days, and the liquid thus obtained filtered and evaporated, beautiful transparent octahedrons were obtained. These proved to be a double salt of phosphate of soda and fluoride of sodium,—

$$3NaO, PO^5 + NaF + 24HO.$$

This salt may also be obtained by digesting for several days at a moderate heat, finely powdered cryolite with a solution of phosphate of soda and caustic soda.

An attempt to produce potash and ammonia salts corresponding to the above gave negative results.

A double salt of arseniate of soda and fluoride of sodium is obtained by fusing arseniate of soda and fluor-spar. To this, arseniate of soda is necessary; and it is better to combine its preparation with this reaction, by fusing together a mixture of nitrate of soda, arsenious acid, carbonate of soda, and fluor-spar. After maintaining the fusion some time, the mass is poured out and digested with water; the salt then dissolves up and may be easily crystallized. The crystalline form of this salt is exactly the same as that of the phosphoric acid compound.

These salts may be considered to belong to the group of alums:—

$Al^2 O^8\ 3SO^8 + KO\ SO^3 + 24HO$. Common alum.
$Na^3 O^3\ PO^5 + Na\ Fl + 24HO$. New salt.
$Na^3 O^3\ AsO^5 + Na\ Fl + 24HO$. New salt.

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Bertagnini has made a series of experiments on the deportment of some organic acids in the animal organism. An abstract of the results appears in the February Number of Liebig's *Annalen*. Crystallized camphoric acid, which is anhydrous, could be taken in doses of half a gramme without any ill effect. Upwards of 12 grms. were taken in the course of two days. The urine had a strongly acid reaction, from which ordinary camphoric acid was obtained. As this differs from crystallized camphoric acid only in containing 2 equivalents of water more, the only change effected had been the assimilation of the water.

Salicylic acid was taken in hourly doses of 25 centigrammes, till in the course of two days 6 grms. had been taken. On the second day a humming in the ears and sensation of numbness were perceived. The urine had, as usual, an acid reaction. Some unchanged salicylic acid was found in it, as well as a quantity of a new acid which contains nitrogen. Its formula is $C^{18} H^9 NO^8$, which is equal to salicylic acid and glycocoll, minus 2 equivs. of water:—

$$C^{14} H^6 O^6 + C^4 H^5 NO^4 - 2HO = C^{18} H^9 NO^8.$$

Salicylic acid. Glycocoll. New acid.

Salicylic acid appears hence to undergo the same change in passing through the organism that benzoic and nitrobenzoic acids do. These are converted into hippuric and nitrohippuric acids; the change being effected, as above, by the assimilation of glycocoll, with loss of 2 equivs. of water. The new acid has been named *salicyluric acid*. By boiling for several hours with fuming hydrochloric acid, salicyluric acid is decomposed into salicylic acid and glycocoll. It is a strong acid, and expels the carbonic acid from carbonates forming salts with the bases, which crystallize readily. It is probably bibasic.

Anisic acid, taken to the extent of 6 grms., passed unchanged through the system and was obtained in the urine. It produced a feeling of heaviness in the stomach.

Piria found that when salicine is treated with nitric acid of a moderate degree of strength, an acid which he named *anilotinic* acid was formed. According to the degree of concentration of the acid, various compounds, as helicine, nitrosalicylic, and picric acids, are obtained. From experiments which he made, Major considered that the anilotinic acid was identical with nitrosalicylic acid. This view is disputed by Piria, who finds that the formation of the acid depends less on the strength of the nitric acid used than on the presence of hyponitric acid. To prepare it, 1 part salicine with 8 parts nitric acid are placed in a well-stoppered vessel and put in a cool place. The nitric oxide generated cannot then escape, and causes the formation of hyponitric acid, which gives to the liquid

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a green colour. After some time, crystals of anilotinic acid separate. Salicine exposed to the action of nitric acid of the same strength in an open vessel is only converted into helicine.

Anilotinic acid has the same composition as nitrosalicylic acid, $HO,C^{14}H^4NO^9$, and the crystallized acid $HO,C^{14}H^4NO^9+3HO$. It has also great similarity in many of its properties. But it differs from nitrosalicylic acid in its solubility in boiling water, and in the salts which it forms with potash, ammonia, and silver.

Neubauer has examined the volatile acid which occurs in the fermentation of diabetic urine. He found that acetic was the only acid formed. Experiments which he made to procure the taurylic acid found by Städeler in fresh normal urine, gave an unfavourable result: phenylic and acetic acids were the only volatile acids which he obtained.

Langenbeck and Städeler investigated the action of the copper salts of the fatty acids on the organism. They found that solution of oxide of copper in fats, as well as the copper salts of fatty acids of high atomic weight, and especially of stearic and oleic acids, have an injurious action on the system, causing vomiting and diarrhœa; but that this action, even in large doses, is not fatal. The copper salts of volatile fatty acids act, on the other hand, as strong poisons, and their action is the more marked the lower the atomic weight of the acid. Acetate of copper has a very poisonous action, which is delayed, but not prevented by a quantity of admixed fats.

Soluble copper salts are decomposed by solution of soap into insoluble stearate and oleate of copper; but in the organism this change is not sufficiently rapid to hinder the poisonous action. Solution of soap is nevertheless the most appropriate antidote, as the vomiting is not prevented. It is best to add to it a small quantity of oil, to prevent the injurious action of the soap on the mucous membrane of the stomach.

Langenbeck and Städeler found that in these experiments the copper was more particularly found in the liver, whence it passes into the bile, and with this reaches the intestinal canal and is removed from the system.

Bœdeker made a series of analyses of cow's milk taken at the various periods of the day in order. The times selected were the morning at 4 o'clock, noon at 12 o'clock, and evening at 7 o'clock. From these analyses, it appears that the increase of fat in the milk from morning to evening is so considerable that the total quantity of solid substances in the evening milk amounts to one-third more than in the morning milk. The quantity of butter in the evening milk is more than double that of the morning.

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The quantity of proteine substances, albumen, and caseine together, remains almost constant. The quantity of sugar of milk is greatest at midday, and decreases towards evening.

The specific gravity of milk is no criterion of its value. A higher specific gravity may indeed be caused by sugar of milk and proteine substances; but a lower specific gravity does not necessarily arise from an increase in the quantity of butter, but also by a greater amount of water.

The importance of this difference, not only for physiological chemistry, but also for dietetics and practical agriculture, is obvious when we consider that a pound of the morning milk of the cow contains about 3 drachms of butter; a pound of the evening milk, on the contrary, 7 drachms.

For the separation of nickel from iron, Schwarzenberg proposes a method founded on Herschel's process; that is, to neutralize the dilute acid solution of the mixed oxides with carbonate of ammonia, and to precipitate the oxide of iron by boiling.

Helkenkamp describes two new double salts of cyanogen with copper and ammonia. To hydrocyanic acid, a solution of hydrated oxide of copper in ammonia is added until the smell of the latter prevails. The mixture is then gently heated, and the addition of the ammoniacal copper solution is continued until the liquid, which at first was yellow, has become blue. After some time green rectangular laminæ, possessing a splendid lustre, appear in the liquid. On analysis, these gave numbers corresponding to the formula

$$2Cu^2\,Cy + Cu\,Cy + 2NH_3 + 2HO.$$

When these crystals are treated at a gentle heat with a mixture of ammonia and carbonate of ammonia, they dissolve into a blue liquid, from which, on cooling, lustrous blue laminæ separate. These are distinguished from the former by containing 2 equivs. of water less. Their formula is $2Cu^2\,Cy + Cu\,Cy + 2NH^3$.

Fuchs had found, that burnt lime exposed to the air formed a compound of caustic lime with carbonate of lime. Wittstein's experiments do not confirm this. He found that caustic lime which was exposed to the air, and from time to time powdered, increased regularly for forty months; on exposure for eight months after that time, no further increase was perceived. It consisted, then, deducting the impurities, of dry carbonate of lime.

In a research which he had undertaken on the Influence of the Nitrates on Vegetation, M. George Ville was led to seek a simple method of determining the nitrates. This he gives in the

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March Number of the *Annales de Chimie et de Physique.* When a solution of nitrate is mixed with excess of a solution of protochloride of iron and boiled, a portion of the iron is oxidized, and at the same time a mixture of deutoxide of nitrogen and hydrochloric acid is disengaged. M. Ville found that when deutoxide of nitrogen, mixed with excess of hydrogen, was passed over spongy platinum heated to redness, the whole of the nitrogen is converted into ammonia, and this may be estimated by a standard solution. This method is applicable even where the nitrate is mixed with considerable quantities of organic matter. It can be used for the determination of nitrates where the quantity of nitrogen does not exceed a centigramme; but when a larger quantity of nitrate is employed, losses are sustained which vitiate the results. Coke washed with hydrochloric acid and calcined in close vessels, Stenhouse's platinized charcoal, and spongy iron may be substituted for the spongy platinum.

If, instead of passing hydrogen and deutoxide of nitrogen over spongy platinum, deutoxide of nitrogen and sulphuretted hydrogen be passed over soda-lime, the whole of the nitrogen is converted into ammonia, which is estimated as above. This method may be used for substances which contain a decigramme of nitrogen. The reaction on which the method depends is thus expressed:—

$$3HS + NO^2 + 2CaO = NH^3 + CaO\,SO^3 + CaS^2 \text{ or } CaS + S.$$

The execution of the method is simple, and does not take long. The protochloride of iron and nitrate are placed in a flask furnished with two tubes. Through one of these, which dips in the protochloride, a current of hydrogen passes which serves to expel all air from the apparatus. The other tube, by which the deutoxide of nitrogen and hydrochloric acid pass off, is connected with a second flask containing some potash which retains the hydrochloric acid. By means of two other tubes, the second flask is placed in connexion with an apparatus for generating sulphuretted hydrogen, and with the combustion tube. The combustion tube contains soda-lime, and affixed to it is a bulb apparatus for absorbing the ammonia. In making the analysis, hydrogen is first passed through the apparatus till all the air is expelled; the combustion tube is then heated, and the current of hydrogen having been moderated, the flask containing the protochloride of iron and nitrate is gently heated. At the same time sulphuretted hydrogen is passed over, so that 3 or 4 centims. of the soda-lime may have become attacked at the time that the disengagement of deutoxide of nitrogen commences. After about ten minutes the action is complete: hydrogen is passed through, the apparatus for absorbing the ammonia is detached, and the ammonia estimated.

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M. Ville describes a very convenient apparatus for obtaining a regular supply of hydrogen or sulphuretted hydrogen, which is a modification of Döbereiner's lamp. He points out, that in the fundamental reaction (that of protochloride of iron on a nitrate) a method of nitrogen determinations, similar in execution and principle to that of Dumas, may be formed. The deutoxide of nitrogen is passed over copper-turnings, and the nitrogen estimated as such. But this is much less general and less certain than the others.

In the same journal M. Béchamp has a paper on Pyroxyline. He considers pyroxyline or gun-cotton as a compound of cellulose, in which 5 equivs. HO are replaced by 5 equivs. NO^5: $C^{24}H^{17}O^{17}5NO^5$, and names it *pentanitrocellulose*. He had found before, that the action of ammoniacal gas gave rise to a new compound, *ternitropyroxyline*, $C^{22}H^{17}O^{17}3NO^5$. By the action of caustic potash on a solution in æther and alcohol of pyroxyline, he obtained an intermediate product, *tetranitropyroxyline*, $C^{20}H^{17}O^{17}4NO^5$. These three nitro-compounds of cellulose differ not only in their definite composition, and in the circumstances of their formation, but also in many of their properties, as the action of solvents, that of reagents, and that of heat.

The action of caustic potash on pyroxyline in the presence of water is different. The final product is in this case sugar. There are intermediate stages in the reaction, but they are not very distinctly marked. The quantity of sugar could not be ascertained, for a great part of it is decomposed into apoglucic and other acids.

Sulphurous acid is without action on pyroxyline. Sulphuretted hydrogen acts, but the products formed are not definite. When pyroxyline is introduced into a strong solution of protochloride of iron, the iron is peroxidized, all the nitrogen is evolved as deutoxide of nitrogen, and the cotton is reproduced, retaining its texture, and all its physical properties. The reaction may be thus written:—

$$C^{24}H^{17}O^{17}5NO^5 + 30FeO + 3HO = 15Fe^2O^3 + 5NO^2 + C^{24}H^{20}O^{20}.$$

From the reproduced cotton, gun-cotton was prepared. By treatment with sulphuric acid it was converted successively into dextrine and sugar.

By the action of protochloride of iron, the primitive matter may be produced from the nitro-compounds formed by the action of nitric acid on starch, gum, mannite, and quercite, as well as from the nitric æthers in general.

There are two distinct series of nitro-compounds; that of which nitrobenzine and analogous bodies are the types, and that of

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which pyroxyline is the type. In the first, hydrogen is replaced by NO^4; and in the last, HO is replaced by NO^5. By the action of ferrous acetate on the first, all the nitrogen remains in the new body formed, as when nitrobenzole is converted into aniline. In the case of the second, the nitric acid is converted into ammonia. Béchamp points out that this reaction may serve as a method of determining ammonia, an idea in the execution of which he has been anticipated by M. Ville.

M. de San Luca found that when about 7000 to 8000 litres of moist ozonized air were passed over potash, nitric acid could be distinctly detected. The air, before being ozonized, was freed from substances held in mechanical suspension, and from nitrogenized substances, by being passed through an apparatus containing potash and sulphuric acid.

LX. *Some Experiments in Electro-physiology.* *By* Prof. Matteucci. *In a Letter to* Dr. Faraday.

My dear Friend, May 1, 1856.

I THINK I have already told you that for some time past I have been making experiments in electro-physiology. Allow me now to communicate to you the results of my work.

I have lately succeeded in demonstrating and measuring the phænomenon which I have called *muscular respiration.* This respiration, which consists in the absorption of oxygen and the exhalation of carbonic acid and azote by living muscles, and of which I have determined the principal conditions and intensity compared with that of the general respiration of an animal, has been studied particularly on muscles in contraction. I have proved that this respiration *increases considerably* in the act of contraction, and have measured this increase.

A muscle which contracts, absorbs, while in contraction, a much greater quantity of oxygen, and exhales a much greater quantity of carbonic acid and azote, than does the same muscle in a state of repose. A part of the carbonic acid exhales in the air, the muscle imbibes the other part, which puts a stop to successive respiration and produces *asphyxy of the muscle.* Thus a muscle soon ceases to contract under the influence of an electro-magnetic machine when it is enclosed in a small space of air: this cessation takes place after a longer interval of time if the muscle is in the open air, and much more slowly still if there be a solution of potash at the bottom of the recipient in which the muscle is suspended. Muscles which have been kept long in vacuum or in hydrogen are nevertheless capable, though in a less degree, of exhaling carbonic acid while in contraction; this proves clearly

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that the oxygen which furnishes the carbonic acid exists in the muscle in a state of combination. According to the theories of Joule, Thomson, &c., the chemical action which is transformed, or which gives rise to heat, is also represented by a certain quantity of *vis viva*, or by an equivalent of mechanical work. I have therefore been able to measure the *theoretical work* due to the oxygen consumed, taking the numbers which I had found for muscular respiration during contraction, and in consequence the quantity of heat developed by this chemical action, and finally this *theoretical work* according to the dynamical equivalent of heat. I have compared this number with that which expresses the *real work* which is obtained by measuring the weight which a muscle in contraction can raise to a certain height, and the number of contractions which a muscle can perform in a given time. It results from this comparison, that the first number is somewhat greater than the second, and the heat developed by contraction ought to be admitted among the causes of this slight difference: these two numbers are therefore sufficiently in accordance with each other.

I completed these researches by some new studies on *induced contraction*, that is to say, on the phænomenon of the irritation of a nerve in contact with a muscle in contraction. A great number of experiments lately made on the discharge of the torpedo, and on the analogy between this discharge and muscular contraction, have led me to establish the existence of an electrical discharge in the act of muscular contraction. The general conclusion to be drawn from these researches is, therefore, that the chemical action which accompanies muscular contraction developes in living bodies, as in the pile or in a steam-engine, heat, electricity, and *vis viva*, according to the same mechanical laws.

Allow me to describe to you briefly the only one of these experiments which can be repeated in a lecture, and which proves the principal fact of these researches, although it is limited to prove that muscles in contraction develope a greater quantity of carbonic acid than those in repose. Take two wide-mouthed glass phials of equal size, 100 or 120 cub. centims.; pour 10 cub. centims. of lime-water (eau de chaux) into each of these phials. Prepare ten frogs in the manner *of Galvani*, that is, reducing them to a piece of spinal marrow, thighs and legs without the claws, which are cut in order to avoid contact with the liquid in the phials. The cork of one of these phials is provided with five hooks, either of copper or iron, on which five of the prepared frogs are fixed. Through the cork of the other phial are passed two iron wires, bent horizontally in the interior of the phial; the other five frogs are fixed by the spinal marrow to these wires. This preparation must be accomplished as rapidly

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as possible, and both the phials be ready at the same instant, and great care taken to avoid the contact of the frogs with the sides of the phials or the liquid. When all is in readiness, with a pile of two or three elements of Grove, and with an electro-magnetic machine such as is employed for medical purposes, the five frogs suspended on the two iron wires are made to contract. After the lapse of five or six minutes, during which time the passage of the current has been interrupted at intervals in order to keep up the force of the contractions, agitate gently the liquid, withdraw the frogs, close rapidly the phials, and agitate the liquid again. You will then see that the lime-water contained in the phial in which the frogs were contracted is much whiter and more turbid than the same liquid contained in the other phial in which the frogs were left in repose. It is almost superfluous to add, that I made the complete analysis of the air in contact with the frogs according to the methods generally employed.

Yours faithfully,

A. Matteucci.

LXI. *Note on an Intuitive Proof of the Existence of Twenty-seven Conics of closest Contact with a Curve of the Third Degree. By* J. J. Sylvester, *Professor of Mathematics at the Royal Military Academy*.*

In general a conic can only be made to have five coincident points with a curve, and if the curve be of the third degree, the conic will of course cut it in a remaining sixth point; but at certain points of the cubic all these six points may come together. How many of these are there, and where are they? This question, which originated with Steiner, who stated the number, and subsequently treated by Plücker, who assigned the position of the points, may be resolved by very simple considerations and without calculation. For if we can succeed in putting the characteristic of the curve (I mean what is commonly, but not altogether commodiously, called "the-left-hand-side-of-the-equation-to-the-curve-when-the-right-hand-side-of-it-is-made-equal-to-zero") under the form $u^3+v(uw+\omega^2)$, it is obvious that the conic $uw+\omega^2$ will intersect the cubic curve in the six coincident points $u^3=0$, $\omega^2=0$.

If now we take for our cubic the reduced form $x^3+y^3+z^3+6mxyz$, and make $x+y+2mz=p$, $\rho x+\rho^2 y+2mz=q$, $\rho^2 x+\rho y+2mz=r$, it may be written under the form

$$(1-8m^3)z^3+pqr, \text{ say } -\mu z^3+pqr;$$

or, if we please, under the form

$$-\mu(z+kp)^3+p(qr+\mu k^3p^2+3\mu k^2p^2+3\mu kz^2).$$

* Communicated by the Author.

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And if we assume k properly, $z+kp$ may be made to touch the multiplier of p, i. e. the multiplier may be made to take the form

$$-\mu(z+kp)^2+p\big((z+kp)v+\omega^2\big).$$

From the symmetry which reigns between x and y, it is obvious *à priori* that any value of k which is rightly assumed for the object in view will make ω (when z is eliminated from it by means of the equation $z+kp=0$) a multiple either of $x-y$ or $x+y$; the latter obviously cannot be true, since such values would make the given cubic a function of $x+y$ and z; the proper values of k will therefore make $x-y=0$, from which, combined with the equation $2x^3+z^3+6mx^2z=0$, the values of $x:y:z$ may be determined. These will be three in number; and as we may write, instead of x and y, ρx, $\rho^2 y$, or ρy, $\rho^2 x$, we obtain three sets of three points, corresponding to p being taken $x+y+2mz$; and consequently, by interchanging z with x and with y successively, we obtain altogether three systems of three sets of three points each; any such factor as $x+y+2mz$ is a tangent to a point of inflexion, and it is clear *à priori* that if the cubic is put under the form $u^3+v(uw+\omega^2)$, since $v=0$ make $u^3=0$, *v can only be* a tangent at an inflexion. Hence the nine sets of three points just assigned are *all* that can be found enjoying the property in question, and it is readily seen that $x-y$ is the straight line containing the three points of intersection in which the second emanant,

$$\left(x'\frac{d}{dx}+y'\frac{d}{dy}+z'\frac{d}{dz}\right)^2\left(x^3+y^3+z^3-6mxyz\right),$$

at the point of inflexion $[x+y=0,\ z=0]$ cuts the given cubic over and above the three coincident points $x+y=0$, $z=0$. In other words, each ternary group of the twenty-seven points in question consists of the three points in which the curve is met by the tangents drawn from a point of inflexion, which agrees with the geometrical construction given by Plücker in Crelle's Journal.

Woolwich, May 3, 1856.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 393.]

June 21, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read :—

"On the ultimate arrangement of the Biliary Ducts, and on some other points in the Anatomy of the Liver of Vertebrate Animals." By Lionel S. Beale, M.B.

In his valuable communication to the Royal Society in 1833,

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Mr. Kiernan describes and figures anastomoses between branches of the biliary ducts in the left triangular ligament of the human liver. The same author considered that the interlobular ducts anastomosed with each other, and communicated with a lobular biliary plexus, although he had never succeeded in injecting this plexus to the extent shown in his figure, neither had he directly observed the anastomoses between interlobular ducts. It must be borne in mind that these observations were made before the liver-cells had been described.

Since the appearance of Mr. Kiernan's paper, various hypothetical views have been advanced by different observers, with reference to the arrangement of the minute biliary ducts and the relation which the liver-cells bear to them. These points, however, have not yet been decided by actual observation.

Müller considered that the ducts terminated in blind extremities. Weber showed that the right and left hepatic ducts anastomosed by the intervention of branches in the transverse fissure of the liver, which he described under the name of Vasa aberrantia.

Krukenberg, Schröder Van der Kolk, Retzius, Theile, Backer, Leidy and others have adopted the view that the liver-cells lie within a network of basement membrane. On the other hand, Handfield Jones and Kölliker describe the liver-cells as forming a solid network, against the marginal cells of which Kölliker believes the extremities of the ducts impinge, while Handfield Jones holds that the ducts terminate by blind extremities.

Henle, Gerlach, Hyrtl and Natalis Guillot look upon the finest gall ducts as communicating with intercellular passages.

Dr. Handfield Jones looks upon the small cells in the extremities of the ducts as the chief agents in the formation of bile, and to the liver-cells he assigns an office totally distinct from this. Busk and Huxley concur in this view, which would place the liver in the category of vascular glands, spleen, suprarenal capsules, &c.

The observations of the author have been made upon the livers of several different animals examined under various circumstances. The results of the examination of injected preparations precisely accord with the observations made upon uninjected specimens some months before. The points which he hopes to establish are as follows:—

1. That the hepatic cells lie within an exceedingly delicate tubular network of basement membrane.

2. That the smallest biliary ducts are directly continuous with this network.

3. That at the point where the excretory duct joins the tubes which contain the secreting cells, it is very much constricted, being many times narrower than the tube into which it becomes dilated.

Lobules.—With reference to the nature of the lobules of the liver, the author offers some remarks. The only liver in which he has been able to detect distinct lobules, consisting of perfectly circumscribed portions of hepatic structure and separated from each

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other by fibrous tissue, is that of the pig. In this liver each lobule has a distinct fibrous capsule of its own, and is separated from its neighbours by the branches of the vessels and duct for their supply.

The lobules of the liver of other animals are not thus separated from each other, but the capillary network and the cell-containing network of one lobule are respectively connected with those of the adjacent lobules at certain points between the fissures in which the vessels and duct lie. In these livers there is not a trace of fibrous tissue between the lobules.

The exceptional liver of the pig, with its distinct lobules, seems to bear in structural peculiarity the same relation to the livers of other animals, as the much-divided kidney of the porpoise bears to the more solid organ of most mammalian animals.

In a *physiological* sense the livers of all vertebrate animals may be said to be composed of lobules; but in a strictly *anatomical* sense this term can only be used with reference to the liver of the pig, and, according to Müller, that of the polar bear. The vessels and duct, at their entrance into the liver, are invested with much areolar tissue, which is continued for a considerable distance along the portal canals; but it gradually ceases as the vessels become smaller, and, with the exception of the liver of the pig above referred to, the lobules are not separated from each other by any areolar tissue, or by any fibrous tissue whatever, neither is any prolonged into their substance. Hence the investment of areolar tissue round the vessels in the portal canals of the liver seems to present no peculiar characters in its distribution. It must be borne in mind, that in the examination of uninjected specimens the small vessels and ducts are liable to be much stretched and torn in manipulation, and, in consequence, a striated appearance is produced which closely resembles fibrous tissue.

Method of preparing specimens.—In order to demonstrate the arrangement of the ducts described by the author, it is absolutely necessary to harden the liver previously. This hardening may be effected by soaking a portion of the liver for some time in strong syrup, or in alcohol, and afterwards rendering the section transparent by soda. The mixture of alcohol and acetic acid recommended by Mr. L. Clarke in his investigations upon the spinal cord, has also been employed, as well as many other solutions which are not described. The fluid to which the author gives the preference is alcohol, to which a few drops of solution of soda have been added.

Method of injecting the biliary ducts.—The following is the method by which, after numerous trials, the author succeeded in effecting this object. Lukewarm water is injected into the portal vein. After a time, when the liver has become fully distended, much bloody water will escape from the hepatic vein, but at the same time it will be remarked that bile escapes from the duct. This bile gradually becomes thinner, and at last nearly pure water flows from the duct, showing that the bile has been washed out. The

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liver is now placed in soft cloths to soak up the water, and after some hours it will be found to have diminished much in volume, and to have a clayey consistence. The ducts are now empty, and may be injected with a carefully prepared prussian-blue injection, to which a little alcohol has been previously added. The mixture is to be well stirred, and after having been carefully strained, it is slowly and cautiously injected into the duct. Plain clear size is next thrown into the portal vein, until the liver has become fully distended with it in every part. Lastly, a little plain size is injected into the duct, the vessels carefully tied, and the liver placed in cold water until the size has set, when very thin sections can be readily obtained with the aid of a sharp knife. The author has tried many other plans of injection, but the above has afforded the most satisfactory results. On one occasion a human liver was successfully injected with four different colours; the portal vein with flake-white, the artery with vermilion, the duct with prussian blue, and the hepatic vein with lake.

Evidence of the existence of a tubular basement membrane in which the liver-cells are contained.

Not unfrequently liver-cells are set free with shreds of delicate membrane attached to them, and this can sometimes be seen to be prolonged either way in the form of a narrow tube.

In certain specimens which have been exposed for some time to the action of dilute soda, the walls of the cells appear to be dissolved and the tubes are seen to be occupied with a highly refractive mass, and their outline is rendered very distinct.

When portions of the cell-containing network are placed in strong syrup or glycerine, exosmose of the water occurs, the diameter of the tubes is much diminished, and their outline becomes distinct, but uneven, in consequence of the shrunken state of the tubes of the network.

At the edge of a very thin section of liver stretching between two capillary vessels, a very thin membrane, recognizable only by the granular matter adhering to it, can sometimes be seen.

The tubes of the network can be distended to a great extent by injection, so that the walls of contiguous tubes meet, while the capillary vessel between them becomes so compressed as not to be recognizable.

The injection often forms a sharp line towards the capillary vessels on either side of the tube in which the cells lie, and gradually shades off towards the centre of the tube.

The cells which escape into the surrounding fluid from injected specimens often have portions of injection adhering to them.

If a section be made at right angles to the intralobular vein, the cells are seen to form lines radiating from the centre towards the circumference of the lobule, as authors have before described. These lines of cells are really tubes of basement membrane, communicating with each other at intervals by narrow branches. In injected specimens the walls of the tube can be demonstrated, and are seen to be distinct from the capillary vessels.

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In the fœtus, the cells are seen to be separated from the cavity of the vessels by two lines separated by a clear space. One of these lines is caused by the outline of the tube containing the cells, the other is that of the capillary wall.

The author supposes that, originally, the liver is composed of a double network of tubes (cell-containing network and capillary network), the walls of which in most situations become incorporated, so that the secreting cells are only separated from the blood by one thin layer of basement membrane, which is very permeable to water in both directions, but the greatest force which can be applied without causing rupture is incapable of forcing bile through it.

Of the contents of the tubular network of basement membrane, and of the arrangement of the cells within it.

Within the tubular network lie the hepatic cells, with a certain quantity of granular matter and cell débris, and, in some instances, free oil-globules and granules of colouring matter. The cells are not arranged with any order or regularity. Some observers have endeavoured to show that the hepatic cells are arranged in a definite manner. Professor Lereboullet, one of the latest writers on this subject (1853), describes the cells as forming double rows. The two rows of cells may be separated by injection, and he gives two diagrams to illustrate their arrangement. The author has never seen anything like this in any liver which has been examined by him. In Mammalia, according to his observation, the cells are for the most part arranged in single rows (human subject, pig, dog, cat, rabbit, horse, seal, Guinea-pig and others), but in some situations two cells lie transversely across the tube, and they may be forced into this position by injection. The cells do not completely fill the tubes, and are not always placed quite close together, being surrounded with granular matter. Injection passes sometimes on one side of the tube, and sometimes upon the other; often it entirely surrounds a cell. In the human fœtus and in the fœtal calf there are two or three rows of cells within the tubes, and this is also the case in the livers of most adult reptiles and fishes which have fallen under the author's observation, and in many parts of the network of the bird's liver.

Of the Ducts of the Liver.

The duct, like the artery, lies close to the portal vein; usually this vessel is accompanied by one branch of the artery and duct, but not unfrequently there are two or three branches of these vessels with the vein.

Anastomosis of the ducts near the trunk from which they come off.—The author observes that the anastomoses between the larger ducts and between the larger branches of the interlobular ducts are pretty numerous in the human liver, but these communications take place only near the origin of the trunks by means of intermediate branches. Different interlobular ducts do not anastomose with each other, but the branches resulting from the division of a small trunk are often connected together.

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In some animals these communications are so numerous, that a complete network is formed at the portal aspect of the lobule, or around a small branch of the portal vein.

Not only are the right and left hepatic ducts connected together by intermediate branches in the transverse fissure of the liver, as E. H. Weber long ago demonstrated, but the branches coming off from these communicate with each other as well as with the trunks from which they come off. These branches are very numerous, and form an intimate network of irregular branched ducts. Similar communications occur between the branches in the portal canals, but they are not so numerous. This arrangement occurs to a less extent in the dog and in the calf, but it is not present in all animals. The author has not been able to demonstrate it in the pig, seal, rabbit, horse, cat and monkey, although he is not prepared to say that, absolutely, no communications take place between the ducts near their origin, in these animals.

Of the glands of the ducts.—The so-called glands are small cavities of a rounded or oval form, or more or less branched, which communicate with the cavity of the duct by a very constricted neck. The simple glands are for the most part situated in the coats of the ducts, so that, when injected, they scarcely project beyond the external surface. These cavities or glands are most easily demonstrated in the pig.

When a small duct from the human liver is laid open, two lines of orifices are seen opening upon the internal surface, as Kiernan described. The great majority of these, however, are not the openings of glands, but almost all of them are the orifices of branches of the duct which communicate with each other in its coats, or just at the point where they leave it. Very few of them are the openings of cæcal cavities, which are very rare in the smaller ducts of the human subject.

Vasa aberrantia.—There are many curious branches of communication between the ducts in the transverse fissure of the liver, which have been well named "vasa aberrantia" by Weber. Theile looks upon all these ducts as anastomosing mucus-glands. The author has seen these ducts in the portal canals, down to those not more than one-eighth of an inch in diameter. They present the same characters as the branches in the transverse fissure, but are not so numerous. The coats of the vasa aberrantia are thinner than those of the ordinary ducts, and, like them, are lined with epithelium, principally of a subcolumnar form. These branches are always surrounded by areolar tissue, in which lymphatics are very numerous. The arrangement of the vessels about the vasa aberrantia is peculiar. The arteries and veins form a network, and each small branch of the artery lies between two branches of the vein, which communicate with each other at frequent intervals by numerous transverse branches, some of which pass over and some under the artery. The author observes that this beautiful arrangement of the vessels occurs in the gall-bladder, in the transverse fissure, and in the portal canals. This disposition of the veins has the effect of ensuring free

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circulation through them under different conditions, as when they are stretched or compressed.

The vasa aberrantia in the transverse fissure of the adult human liver are nearer to the branch of the portal vein than to the hepatic substance, and can be readily removed without any of the latter. A few small straight branches are sometimes observed to come off from the vasa aberrantia and to enter the hepatic substance. In the fœtus, on the other hand, the vasa aberrantia are fewer in number, their course generally is more direct, they lie so close to the hepatic tissue that they cannot be removed unless a portion of the latter is taken away with them, and very many of the branches can be traced into the hepatic substance.

The author regards the vasa aberrantia in the adult liver in the light of altered secreting tubes, and believes that at one time they formed a part of the secreting structure of the liver. At the termination of intrauterine life the portal vein increases in size, and the pressure thus produced may account for the gradual wasting and partial disappearance of the hepatic substance closely surrounding it. In the very thin edge of a horse's liver, which consisted principally of areolar tissue, the gradual alteration of the ducts and ultimate complete disappearance of the secreting cells was traced. Upon the surface of the portal vein in the rabbit's liver the transitional stages between the compact lobule of secreting structure and the branches of the vasa aberrantia have been well seen.

Function of the glands and vasa aberrantia.—It has always been considered that the office of the ducts was to secrete the mucus of the bile, and a similar function was assigned to the vasa aberrantia by Theile. It seems to the author that a cavity communicating with a tube by a neck of less than $\frac{1}{5000}$th of an inch in diameter, cannot be well adapted for pouring out a viscid, tenacious mucus. If these cavities contained mucus, the injection would not enter them so readily as it does, nor is it easy to conceive how the mucus poured out by these little glands would become thoroughly mixed with the bile as it passes along the ducts. Again, the bile of the pig, in which animal these glands are very abundant, does not contain more mucus than the bile of the rabbit, in which they are few in number and only found on the largest branches of the duct. The vasa aberrantia do not possess any characters which, in the opinion of the author, justify the inference of their being mucus-glands. He regards the little cavities in the coats of the ducts (glands of the ducts) and the vasa aberrantia as reservoirs for containing bile, whilst it becomes inspissated and undergoes other changes. By these cavities in the ducts with thick walls, the bile is brought into close relation with the vessels which ramify so abundantly upon the external surface of the ducts.

Of the finest branches of the duct, and of their connexion with the cell-containing network.

Mammalia.—In well-injected preparations the smallest branches of the duct can be readily traced up to the secreting cells of the

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lobules. In most Mammalia, but not in the pig, a few of the finest branches of the duct can be followed for some distance beneath the surface of the lobule. These branches appear to lie amongst the secreting cells, but are not connected with them. They become continuous with tubes of the cell-containing network at a deeper part, while those secreting tubes nearer the surface of the lobule are connected with branches of the duct which do not penetrate.

In many animals, particularly in the rabbit, and to a less extent in man and in the dog, the smallest branches of the duct are connected together so as to form a network, which is continuous with that in which the secreting cells lie.

In the pig, the small ducts are, as it were, applied to the surface of the lobule; from these smaller branches come off, which penetrate the lobule and are immediately connected with an intimate network, which lies partly in the capsule of the lobule itself. This network is continuous with, and may be looked upon as the most superficial portion of, the cell-containing network. In a perfectly normal state it contains only oil-globules and granular matter; but when the liver is fatty, it is found to contain liver-cells loaded with oil. From such a liver the author has a very beautiful preparation, in which the continuity of the very narrow duct with the wide tubes of the network, distended with large cells containing oil, can be well seen. The duct and the tubes in which the secreting cells lie, both contain a little injection.

The author has succeeded in demonstrating the communication between the ducts and cell-containing network in several mammalian animals, as well as in the human subject, by injecting the ducts in the manner described. Of these, the seal, hedgehog, rabbit and Guinea-pig have afforded the best specimens.

In *Birds*, the continuity in injected specimens has been traced in the common fowl and in the turkey. The quantity of epithelium in the ducts of birds forms a great obstacle to the passage of the injection, and from their extreme tenuity, the capillaries do not bear the preliminary injection of much water.

Reptiles.—The author has seen the continuity between the ducts and cell-containing network, in an uninjected preparation of the newt's liver, and in an injected liver of the adder.

Fishes.—In consequence of the very fatty nature of the liver of fishes, it was found to be very difficult to harden it sufficiently to cut thin sections. The frequent presence of entozoa and their ova, renders it difficult to inject the ducts. The author succeeded in injecting the ducts and part of the cell-containing network in the sturgeon and in the Lophius, and in one instance, those of the very fatty liver of the cod. The continuity was also traced in an uninjected liver of the common flounder. The injection often passes a certain distance into the finer ducts of fishes, but cannot be forced into the cell-containing network. In this way the appearance of blind terminations to the ducts is produced, as the continuity of the tube cannot be traced beyond the point at which the injection stops.

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The continuity of the finest ducts with the cell-containing network has been demonstrated in all classes of Vertebrata, both in injected and also in uninjected specimens. In all the livers of vertebrate animals which have been examined, the duct becomes much narrowed at the point where it joins the network in which the cells lie. The arrangement of the small ducts varies somewhat in different animals. Sometimes a network of minute ducts is formed, which is continuous with that in which the cells lie. In other instances the communications between these terminal ducts are very few in number, or are altogether absent. Upon the latter point the author does not express himself positively, as he is sure that in the most perfect injection which he has been able to make, the whole of the numerous branches of the minute ducts have not been injected; and from observations upon these specimens alone, he feels that only a very imperfect idea can be formed of their number or of their arrangement.

Diameter of the ducts.—A table is given, showing the thickness of the coats of the ducts in different parts of their course. The walls of the smallest ducts are composed entirely of basement membrane, and are often not more than the $\frac{1}{5000}$th of an inch in diameter in the uninjected state. In the pig, the diameter of the smallest ducts containing injection was about the $\frac{1}{3000}$th of an inch; in the human subject, about the $\frac{1}{2500}$th; in the seal, $\frac{1}{3000}$th; and in some fishes not more than the $\frac{1}{8000}$th of an inch. It may be remarked, that this narrowing of the excretory duct, just before it becomes continuous with the secreting portion of the organ, is seen in the kidney and in other glands.

Epithelium of the small ducts.—The epithelium lining the smallest ducts presents very similar characters in different animals. The small cells are for the most part oval or circular in form; sometimes they are angular, which probably results from pressure or stretching of the ducts in the preparation of the specimen; sometimes the smallest ducts appear to be entirely filled with epithelium; in other instances the cells are very sparingly and irregularly scattered over the interior of the tube, while frequently no cells whatever can be distinguished.

The author believes that, in a perfectly normal state, the minute ducts are lined by a single layer of delicate epithelial cells.

This ductal epithelium does not pass gradually into the secreting epithelium, but ceases at the point where the latter begins. Hepatic cells are sometimes seen in tubes lined with this ductal epithelium, but probably their presence is the result of accident. In these cases the ducts are of course much stretched or dilated *.

With reference to the relation of the ductal epithelium to the secreting cells of the liver, the author observes that a very similar arrangement occurs in the gastric glands. The secreting epithelium is alone found in the lower part of the gland (stomach tube), while the ductal portion of the gland is lined with columnar epithelium.

* Mr. Wharton Jones has also seen hepatic cells in the small ducts.—*Phil. Trans.*

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The secreting cells appear to occupy the entire cavity of the tube, and are not arranged in any order; so that the secretion, having escaped from the cells, must pass off towards the duct by the slight interstices between them. A similar disposition of the secreting epithelial cells occurs, but in a less remarkable degree, in some other glands; as the pancreas, lacteal, sebaceous, and sweat glands.

The conclusions to which the author has arrived may be summed up as follows:—

1. That the liver of vertebrate animals essentially consists of two solid tubular networks mutually adapted to each other. One of these networks contains the liver-cells, and the other the blood.

2. The cell-containing network is continuous with the ducts. The small delicate epithelial cells lining the latter channels contrast remarkably with the large secreting cells, which are not arranged in any definite manner within the tubes of the network.

3. The duct is many times narrower than the tubular network at the point where it becomes continuous with it.

4. Injection passes sometimes on one, and sometimes on the other side of the tube, or between the cells, when two or more lie across the tube. Often, a cell becomes completely surrounded with injection. As injection can thus be made to pass readily *from* the ducts into the network and around the cells, it follows that there can be no obstacle to the passage of the bile along the same channels in the opposite (its natural) direction.

5. In some animals, the most minute ducts are directly connected with the tubes of the cell-containing network; of these branches, some pass amongst the most superficial meshes to join the network at a deeper part. In other animals the finest ducts first form a network which is continuous with that containing the liver-cells.

6. The interlobular ducts do not anastomose, but the branches coming off from the trunk are often connected with each other, as well as with the parent trunk, near their origin from it.

7. The walls of the smallest ducts are composed of basement membrane only. The thick complex coat of the larger ducts contains within it small cavities (the so-called glands of the ducts), by means of which the bile in these ducts would be brought into close proximity with the arteries, veins and lymphatics, which are very abundant wherever the ducts ramify.

8. The office of the vasa aberrantia, which are so numerous in the transverse fissure of the human liver and in the larger portal canals, appears to be similar to that of the cavities in the walls of the ducts. It is worthy of remark, that the network of vessels ramifying so abundantly in the coats of the gall-bladder, in the transverse fissure, and in the larger portal canals, are arranged in a similar manner, each branch of artery being accompanied by two branches of the vein.

9. The liver is therefore a true gland, consisting of a formative portion and a system of excretory ducts directly continuous with it. The secreting cells lie within a delicate tubular network of basement membrane, through the thin walls of which they draw from the blood the materials of their secretion.

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"Report made to the President and Council of the Royal Society, of Experiments on the Friction of Discs revolving in Water." By James Thomson, Esq., C.E., Belfast.

[A Committee of the British Association for the Advancement of Science, consisting of James Thomson, Esq., C.E., and William Fairbairn, Esq., C.E., F.R.S., having been appointed "to make Experiments on the Friction of Discs revolving in water, with especial reference to supplying data wanted in calculations relative to the action and efficiency of Turbine Water-Wheels in general, and of Centrifugal Pumps; and also to make an experimental investigation relative to the action and efficiency of Centrifugal Pumps in general, and the amount of improvement derivable in them by the employment of an exterior whirlpool;" a sum of £50 from the Government Grant of 1853 was allotted by the Council of the Royal Society in aid of the inquiry. The experiments, as originally contemplated, have been arranged and conducted by Mr. Thomson, and the present Report of his progress is here inserted by order of the President and Council for the information of the Fellows.]

In last year's Report of the Committee it was stated, that an apparatus for making experiments on the friction of discs revolving in water had been constructed, and that experiments had been commenced with it. I have now further to state respecting the experiments for which that apparatus was adapted, that I have since got them completed and carefully arranged for the purpose of obtaining from them laws applicable for practical use.

I now beg to lay before the Royal Society, as a brief statement of the most essential results, the following general equation to show the relation between the velocity of revolution of the disc, the diameter of the disc, and the mechanical work consumed in friction:—

$$z=\frac{y^3d^5}{90{,}000},$$

in which d = diameter of the disc in feet,
y = number of revolutions of the disc per minute,
and z = number of foot-pounds of mechanical work consumed per minute.

This equation is based on experiments which range for the most part between the limits $yd=192$ and $yd=518$, and may be used with confidence, as sufficiently correct for most practical purposes, if the product of the number of revolutions per minute and the diameter of the disc in feet be between those limits. It is to be observed that the friction is slightly affected by the width of the water space within the case, and the coefficient 90,000 stated in the formula above is, for simplicity in the present brief report, taken between the coefficients obtained by two sets of experiments with different widths. A full report on the experiments already made, explaining the manner of conducting them and stating the detailed results, would be rather lengthened, and would require drawings and diagrams, for all of which I have carefully preserved the requisite data; but before proceeding to put these in form suitable to be submitted to the Royal Society, I am desirous of prosecuting the

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remainder of the very interesting and important experiments which have been entrusted to me,—that portion of the whole, namely, which relates especially to centrifugal pumps. I have also to state, that if my engagements permit, I should be desirous of proceeding with a renewed and more extended set of experiments on the friction of discs, with an apparatus depending on the same leading principle as that which I have already used,—a principle which on trial has been found remarkably well suited for the desired purpose. For the attainment of greater accuracy and of a wider range of the experiments, it seems to me that no better method of procedure could be adopted, than to follow the same leading principles, with an apparatus of rather more refined construction, involving such improvements in details as have been suggested by the experience gained in the course of the experiments already made, and for the sake of greater steadiness of motion, worked by steam power instead of the hand of an operator. Should I have it in my power to conduct this renewed set of experiments, a detailed account of them will be preferable to a detailed account of those already made.

In respect to the experiments on Centrifugal Pumps, I have to say that I have prepared plans for an experimental apparatus on principles which I consider are peculiarly well suited for the attainment of useful and accurate results, and that I intend to proceed with the experiments as soon as my engagements shall permit.

I have further to state, that from the Experimental Fund of £50 granted by the Royal Society, the entire outlay as yet incurred has been £6 5*s.* 9*d.*, leaving a balance of £43 14*s.* 3*d.* for the more extended experiments yet remaining to be made.

JAMES THOMSON.

Belfast, April 13, 1855.

Nov. 22.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

The reading of Dr. Faraday's paper, "Experimental Researches in Electricity—Thirtieth Series," was resumed and concluded.

The following is an abstract:—

* § 38. *Constancy of differential magnecrystallic force in different media.*—That a magnecrystal formed into a sphere (or some equivalent shape, so that mere length should have no influence) sets with the same force in the magnetic field, whatever the magnetic nature of the medium around it, has been shown generally, and for a few cases, on former occasions. The author was under the necessity of verifying and enlarging the old results; and upon employing the following magnecrystals, namely bismuth, tourmaline, carbonate of iron, red ferroprussiate of potassa, and also compressed bismuth, surrounded in succession by the following media,—phosphorus, alcohol, oil, camphine, water, air, and saturated solution of protosulphate of iron, he found the result to be the same as before. The mode of estimating the set was as follows:—The selected crystal being suspended in the magnetic field by a torsion-wire, right-handed force was then slowly applied by the revolutions of the torsion-head above,

* Series XXIX. is published in the Phil. Trans. for 1852, p. 137.

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until the crystal being gradually carried round, attained that position at which any additional torsion-force would cause it to advance suddenly and considerably; this position was called the upsetting point; then left-handed torsion was put on until the like point was attained in the opposite side: the amount of the revolution of the torsion-index from one upsetting point to the other, minus the angle between the upsetting points, was considered as the measure of the set of the crystal under the constant magnetic force employed.

As the setting force of a crystal remained constant for any surrounding medium, it was evidently possible to select a crystal and a medium such, that in one position the crystal would be attracted, and in another, at right angles to the first, be repelled in the same medium. This case was realized with the paramagnetic red ferroprussiate of potassa and a solution of sulphate of iron, and also with the diamagnetic crystal carbonate of lime and diluted alcohol. A crystal was sought for amongst the ferrocarbonates of lime having this relation to the assumed natural zero presented by a vacuum or carbonic acid; but this case was not realized.

§ 39. *Action of heat on magnecrystals.*—When magnecrystals, subjected to the same constant magnetic force, were raised or lowered to different temperatures, it was found that the setting force was affected; and at all temperatures from 0° F. upwards the force diminished as the temperature became higher. Thus the torsion-force of a crystal of bismuth at 92° being 175, was at 279° diminished to 82; that of a tourmaline, by passing from the temperature of 79° to 289°, was so far diminished; that the power at the lower temperature was nearly double that at the higher. A like result occurred with carbonate of iron, and also with compressed bismuth. In all these cases the bodies resumed their first full power on returning to lower temperatures, nor was there any appearance of magnetic charge in any part of the range of observations. Between 32° and 300° the force of bismuth appeared to alter by regular equal degrees; but with tourmaline and carbonate of iron the change was greatest for an equal number of degrees at the lower temperatures. At a full red heat, however, both tourmaline and calcareous spar retained a portion of their magnecrystallic force or condition, and so did carbonate of iron up to that temperature at which it was decomposed.

It is known that pure calcareous spar points with its optic axis equatorially, but that calcareous spar containing a trace of iron points with its optic axis axially. Calcareous spar retains its magnetic characters at very high temperatures, but carbonate of iron and oxide of iron lose almost the whole of their magnetic force at a dull red heat. It was therefore expected that a ferrocarbonate of lime crystal might become absolutely reversed in condition by change of temperature, and this was found to be the case: at low temperatures the optic axis pointed axially, and at high temperatures equatorially; and that through any number of changes, as the temperature of the crystal was alternately lowered and raised.

§ 40. *Effect of heat upon the absolute magnetic force of bodies.*—

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Results were sought for, by which the magnetic force of bodies, already examined in the condition of magnecrystals, might be compared with the whole paramagnetic or diamagnetic force of the same bodies taken in the granular or amorphous state; but they were not satisfactory. The carbonate of iron gave the most distinct results; and in its case the change of power by change of temperature was not the same for the two conditions. An examination of the three metals, iron, nickel, and cobalt, at temperatures between 0° and 300° F., gave a very interesting result, which the author is not aware has as yet been noticed. As the temperature rises, the force of the nickel diminishes, the force of the iron remains constant, the force of the cobalt increases; these facts suggest that there is a temperature at which the magnetic force is a maximum, and above or below which it diminishes. The order with the three bodies accords perfectly with that in which they lose the chief amount of their magnetic power, for much loss occurs with nickel at the temperature of boiling oil, with iron at a dull red heat, and with cobalt at a temperature near that of melting copper.

GEOLOGICAL SOCIETY.

[Continued from p. 398.]

April 23, 1856.—Daniel Sharpe, Esq., President, in the Chair.

Mr. G. Poulett Scrope, M.P., F.G.S., read a paper "On the Mode of Production of Volcanic Craters, and on the Nature of the Liquidity of Lavas."

The author referred to the two works published by him thirty years since, namely, 'On Volcanos' in 1824, and 'On the Volcanic Formations of Central France' in 1826; being desirous of calling attention to certain theoretical views developed in both, which were either controverted at the time or met by opposite theories, but which he believes the progress of inquiry has since tended to confirm.

The first point insisted on is the formation of all volcanic cones and craters by the simple process observed in habitually active volcanos, namely, the eruptive ejection of lavas and fragmentary matter from a volcanic vent; the accumulation of which around it cannot fail to give rise to the cone-shaped mountain so characteristic of a volcano, and to the crater usually contained in it. The author showed, by the history of Vesuvius, that the cone of that mountain has, within the last hundred years, been at least five several times emptied by explosions of a paroxysmal character, and as often refilled by the products of subsequent minor eruptions; while throughout this time the exterior of the cone has been gradually increasing in bulk, and the old crater of Somma as gradually being filled up, by accretions from the volcanic matter ejected beyond the lip of the Vesuvian crater. He refuses to believe that any other process originally formed the outer cone and crater of Somma, than that which he and others have seen to be continually augmenting the inner cone of Vesuvius, and which before his eyes in 1822 scooped out of its heart a crater concentric to that of Somma, three miles in circumference and some 2000 feet in depth.

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And generally of other great craters, ancient or modern, such as Palma, Santorini, the Val de Bove, &c., he considers that no argument in favour of their having any other than a similarly "eruptive" origin can be derived from the fact of their dimensions exceeding those of the crater of Vesuvius. The authentic accounts of enormous quantities of ejected pumice, scoriæ, or ashes thrown out by many eruptions from Polynesian or American volcanos, reaching to distances of above a thousand miles, and of course spreading over the whole intermediate space, to a thickness sometimes of 10 or 12 feet at more than twenty-five miles from the volcano, would amply account for the dispersion, by explosive eruptions, of the contents of the largest craters ever observed.

At the same time the author guards himself from being supposed to have ever denied that some amount of elevation has taken place in the external cone of a volcano through the occasional injection of lava from within into rents broken across its framework, and hardened into dykes, which may be called a process of gradual distension. This, in fact, was suggested by him in 1824. All he contends against is the theory of Von Buch, that volcanic mountains are the result of the elevation of nearly horizontal beds of lava and conglomerates by some sudden expansion. He maintains, on the contrary, that the growth of a volcano by accretion, through eruptive ejections on the exterior, and partial distension from within, is a gradual, though intermittent, normal process, which may be watched almost like the growth of a tree.

The author next referred to the opinion published by him in 1824, that the liquidity of the stony and crystalline lavas (excluding the vitreous varieties) at the time of their protrusion, is owing, not to complete fusion, but to the entanglement between their component granular or crystalline particles of some fluid, chiefly water, at an intense heat of course, but unvaporized by reason of the extreme pressure to which they are subjected while beneath the earth, and escaping in vast bubbles of steam, when, by the opening of a fissure of escape, its discharge is permitted, and also by a kind of exudation through the pores and crevices of the expelled lavas as they cool.

The author originally extended this theory of the combination of aqueous with igneous agency in lavas to all the crystalline plutonic rocks, which he considered to be derived from a mass existing beneath the crust of the globe under the above circumstances, in a state of extreme tension, such as on the occurrence of any sufficient local relaxation of the restraining pressure from above, or increase of temperature from within, must occasion its partial intumescence, and the consequent fracture and elevation of the overlying rocks, with or without extravasations of the intumescent crystalline matter through rents, in the form either of volcanic eruptions, or the protrusion of the granitoidal axes of mountain chains.

These ideas on the character of the liquidity of lavas and the hypogene crystalline rocks, promulgated by the author in 1824–26, were considered unchemical at that time and little regarded. They have, however, of late been reproduced by M. Scheerer of Freiberg

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and adopted by M. Elie de Beaumont, and have received much confirmation from recent researches into the conduct of water under pressure at high temperatures, its power of taking silex into solution, &c.

The author further asks the attention of geologists to the ideas developed by him in the same early works, and founded on actual and careful observations, as to the change of position occasioned in the component crystals of a matter moving in the pasty state here attributed to lavas and other plutonic rocks, during their emission or elevation under extreme pressure. He produced examples from the ribboned trachytes and pearlstones of Italy, Hungary, and Mexico. He considers gneiss to be granite elongated by a powerful lateral squeeze, probably at the time of its expulsion; and mica-schist to be the extreme result of the same action upon the lateral bands or selvages of the extruded mass or great dyke. This he thinks a more probable origin than the usual metamorphic theory of the melting and reconsolidation of sedimentary strata, though the one does not wholly exclude the other. At all events he considers the evidence presented in the peculiarities of texture, structure, and position of the laminated crystalline rocks to be conclusive as to their having been squeezed, flattened, and drawn out in the direction of their upcast, and attributes this process to the same elevatory movements which have thrust them up, and often forced them into wrinkled foldings on the grandest as well as on the most minute scale. To this same rearrangement of their crystalline plates or flakes under pressure he attributes also their lamellar cleavage. He refers to Mr. Sorby's recent paper and experiments on slaty cleavage as confirming these views. The paper ends by recommending the more earnest study of the dynamics of geology, which has in this country been perhaps of late years somewhat neglected.

May 7, 1856.—Daniel Sharpe, Esq., President, in the Chair.

The following communications were read:—

1. "On some supposed Foot-marks in the Millstone-grit of Tintwhistle, Mottram en Longdendale, Cheshire." By E. W. Binney, Esq., F.G.S.

In a quarry in the lowest portion of the millstone-grit,—certainly 1000 feet down in that formation, and very near the underlying limestone-shale,—a series of five large impressions, lying in a straight line and nearly on the rise and dip of the strata, were met with. The strata dip towards 80° west of south at an angle of 12°. The dimension of the impressions somewhat varies, but they are much of a size. Two of them, the longest, measure each 13 inches in length at their bottom, and 17 inches above; their breadth being respectively 4 and 3½ inches at the bottom, and 8 and 9 inches above; their depth is about 3 inches. The distances between the impressions, measuring from the middle of one to the middle of another, is 2 feet 10½ inches in every instance. The impressions differ slightly in shape, but the bulk of the wet sand that had been originally displaced out of the holes was the same in each instance, whether the impressions were deep and short, or shallow and long; and the sand removed was

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forced up on the western side, and has the aspect of having been at least twice subjected to pressure; as if one portion of the semifluid mass had been displaced, and another subsequently pushed partly over it, by successive footsteps. The author supposes these impressions to have been made by the same kind of animal as that which gave rise to the foot-marks on the Permian sandstones of Corncockle Muir, and which has been termed *Chelichnus* by Sir W. Jardine. The Tintwhistle tracks are referable to a much larger animal than even the *C. titan* of Jardine, and Mr. Binney proposes to call it *C. ingens*.

2. "On the Lignite deposits of Bovey-Tracey, Devonshire." By Dr. J. G. Croker. Communicated by the President.

The author first described the physical features of the basin, surrounding the junction of the Teign and Bovey Rivers, in which these beds of lignite and their associated clays (used in pottery) are found. The lignite-beds come to the surface at Bovey Heath towards the north-western margin of the basin; they underlie towards the south-east about 11 inches in the fathom, and are covered by clays and gravels; their vertical thickness is about 100 feet. In the upper portion of the lignitic series are several (five and more) beds of loose lignite, covered and mixed with variously-coloured clays and granitic detritus; a ferruginous sandy clay, 9 feet thick, succeeds, which is followed downwards by ten beds of "good coal" or lignite, separated by bluish clay-beds and worked for fuel.

Fir-cones, referable to the Scotch-fir (*Pinus sylvestris*), have been found in one of the uppermost layers of loose lignite. Large flabelliform leaves also are represented by fragments 2 feet long and 20 inches wide in some of the higher beds, together with tangled masses of vegetable remains. In the second and fourth beds of good coal (the latter about 80 feet from the surface) the lignite abounds with the little seeds lately described as *Folliculites minutulus* by Dr. Hooker in the Society's Quarterly Journal. The lignite generally is composed of compressed coniferous wood, and retin-asphalte is locally abundant. The Bovey basin is about 60 feet above the sea-level, and was almost a swamp until it was drained within the last ninety years. A peat-deposit, in which fir-timber is found, covers the lignites towards the south.

The author also referred to the extensive denudation that the district has undergone, and pointed to the Dartmoor granitic tract as the source of the clays of the lignitic deposits. He also noticed the several writers who have treated of the lignites and the geology of the neighbourhood. Lastly, Dr. Croker supplied some notes on the local occurrence of the numerous varieties of rocks and minerals in the vicinity of the Teign, such as ores of lead, manganese, and iron, also labradorite, schorl, &c., all of which, as well as the lignite and its vegetable remains, were illustrated by a large series of specimens.

3. "Notice of some appearances observed on draining a Mere near Wretham Hall, Norfolk." By C. J. F. Bunbury, Esq., F.G.S.

About Wretham, six miles north of Thetford, are several meres, or small natural sheets of water, without any outlet. One of these, about 48 acres in extent, has been lately drained by machinery, for

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the purpose of obtaining the black peaty mud forming the bottom, and using it as manure. This black mud is in parts above 20 feet in depth, and may be described as vegetable matter in a more complete state of decomposition than ordinary peat. At a depth of about 15 feet in this peaty deposit occurs a distinct horizontal layer of compressed but undecayed moss, from 2 to 6 inches thick. The moss is sufficiently well preserved to be recognized as the *Hypnum fluitans*, common in bogs and pools throughout the British Isles, and often growing in dense masses in shallow water. The bed of moss is of considerable extent, though not occurring everywhere within the area of the mere. While wet and fresh it is of a bright rusty-red colour, and turns to a yellow-brown when dry. Numerous horns of the Red Deer were found in the peat above the moss-bed, and seldom at a greater depth than 5 or 6 feet from the surface; many of the antlers were of large size, and some appeared to have been cut with a saw. The black peaty mud beneath the moss is identical with the upper portion, and rests on a light grey sandy marl. No shells were observed; but trunks of trees, probably birch and oak, are found. Local seams of sand occur, and occasional stones of flint and quartz, resembling the gravel of the country. Numerous posts of oak-wood, shaped and pointed, were also found standing erect, and covered up by the peat. From the above facts it appears that, a great part of the upper peaty mud having been accumulated before the Red Deer became extinct in this part of England, the moss-bed must consequently have been formed at least some centuries ago; and that, although the few mosses experimented upon by Dr. Lindley decomposed rapidly, yet the aquatic mosses, judging from the fossil bed of moss above described, are not rapidly destroyed by exposure to moisture, and that some other explanation must be sought for to account for the great want of *Musci* in the strata deposited in former geological periods.

4. "Analysis of the Cleveland Iron Ore." By A. Dick, Esq., Metallurgical Laboratory, School of Mines. Communicated by Dr. Percy, F.G.S.

The ore was weighed after drying at 100° C.

Protoxide of iron	39·92
Peroxide of iron	3·60
Protoxide of manganese	0·95
Alumina	7·86
Lime	7·44
Magnesia	3·82
Potash	0·27
Carbonic acid	22·85
Phosphoric acid	1·86
Silica, soluble in hydrochloric acid	7·12
Sulphuric acid	trace
Bisulphide of iron (iron-pyrites)	0·11
Water in combination	2·97
Organic matter	trace
Residue, insoluble in hydrochloric acid	1·64
	100·41

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Composition of the residue insoluble in hydrochloric acid:—

Silica, soluble in dilute caustic potash....	0·98
Silica, insoluble in dilute caustic potash..	0·52
Alumina with a trace of peroxide of iron..	0·10
Titanic acid, about	0·03
Lime	trace
	1·63

The ore contains no metal precipitable by sulphuretted hydrogen from the hydrochloric acid solution.

In the residue insoluble in hydrochloric acid, minute, bright, black crystals were detected, which were proved to contain titanium, and were supposed to be anatase. Prof. Miller of Cambridge has been able to measure certain of the angles, and found them to be identical with similar angles of anatase. The discovery of this mineral in the Cleveland ore is at least a point of considerable mineralogical interest, and may possibly furnish some additional indication of the nature of the rock from which it was derived.

The silica in the insoluble residue exists, it will have been observed, in two states, about two-thirds being soluble in dilute caustic potash, and one-third insoluble in that solvent. The rounded white particles, which, according to Bowerbank, have a truly oolitic or concentric concretionary structure, are entirely formed of the soluble silica.

The silica which existed in the hydrochloric acid solution was that which was present in a state of combination in the ore, probably with both protoxide and peroxide of iron; and the peculiar greenish-grey colour of the ore was doubtless due to the presence of this silicate of the mixed oxides of iron, just as the colour of the green particles in the so-called greensand is believed to be due to the like cause.

The proportion of phosphoric acid in the ore is comparatively large, and may be easily accounted for by the fossiliferous character of the ore. The quality of the iron smelted from this ore would certainly be very sensibly affected by the proportion of phosphorus, and probably also by the silica existing in a state of combination.

5. "On the occurrence of Coal near the City of E-u in China." By the Rev. R. H. Cobbold. Forwarded from the Foreign Office.

The coal is worked by shafts and galleries in the hills near E-u, a third-class city, in the prefecture of King-hua, from which it is distant forty English miles by water. The pits are from 300 to 500 feet deep. The coal is bright, and not bituminous.

ROYAL INSTITUTION OF GREAT BRITAIN.

April 4, 1856.—"On the Measurement of the Chemical Action of Light." By Henry E. Roscoe, Esq., B.A., Ph.D.

No attempt has been made, up to the present time, accurately to measure the changes brought about in chemical substances by the action of the solar rays.

The peculiar action of light on chemical bodies was first observed by Scheele on chloride of silver. Since that time the subject of the chemical action of light has attracted a large amount of attention, as the present perfection of the arts of the daguerreotypist and photographer fully testify. Although we possess so

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many facts concerning the chemical action of light, this branch of science has only as yet arrived at that first or qualitative stage of development, through which every science must pass. The laws which regulate these phænomena are unknown to us, and we possess no means of accurately measuring the amount of the decomposition effected by the light.

The speaker proceeded to describe the results of a series of experiments carried on by him in conjunction with Professor Bunsen, which had for their object,—

1. To determine the laws which regulate the chemical action of light;
2. To obtain a measure for the chemically active rays.

When aqueous solutions of chlorine, bromine, or iodine are exposed (under certain conditions) to the direct solar rays they are decomposed, the corresponding hydracid being formed, and the oxygen of the water liberated. The difference between the amounts of free chlorine, bromine, or iodine, contained in the liquid before and after exposure to light, gives the quantity of the substance decomposed during the isolation. Now it was found that this quantity of chlorine, bromine, or iodine which disappeared, was not proportional to the time of exposure to the light; in twice the time, for instance, less than twice as much substance was decomposed. The relation between the amount of light and the amount of decomposition was found in this case not to be a simple one.

This anomalous action may be explained even from a theoretical point of view. Chemical affinity is the resultant of all the forces which come into play during the reaction; hence it is not only the interchanging atoms which influence the result, but also those atoms which, without taking part in the decomposition, surround those actively engaged. The so-called catalytic phænomena show this action in a striking manner. To apply this general principle to the special case before us; we have to begin with pure chlorine water; after the first action of the light, however, hydrochloric acid is formed, hence the composition of the solution is altered, and a different result must be expected. This theoretical conclusion was verified by experiment. Chlorine water, to which 10 per cent. of hydrochloric acid was added, did not suffer any decomposition by an exposure of six hours to the direct sunlight; during which time the same chlorine water, without previous addition of hydrochloric acid, lost nearly all the free chlorine which it contained *.

In order then to obtain a true measure of the action of light on any chemical substance, it is necessary that the body formed by the decomposition should be removed from the sphere of action. This cannot be done with chlorine water; a new sensitive substance was therefore employed.

Equal volumes of chlorine and hydrogen gases when exposed to the direct sun-light unite with explosion; in diffuse light, the action proceeds gradually. In presence of water the hydrochloric acid formed by the combination is immediately absorbed, and thus withdrawn from the sphere of action, and the diminution of the volume of the mixed gases arising from this absorption gives an exact mea-

* Poggendorff's *Annalen*, xcvi. 373; and Quarterly Journal of Chemical Society, Oct. 1855.

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sure of the amount of action effected by the light. The diminution in volume of the gas measured by the rise of water in a graduated tube was found to be regular, proving *that when the light is constant the amount of action is directly proportional to the time of exposure.*

The relation between the amount of action and the amount of light was experimentally determined, by allowing known quantities of diffuse light to fall upon the sensitive gas. Experiments thus conducted showed *that the amount of action is directly proportional to the amount or intensity of the light.* These simple relations were observed by Dr. Draper, of New York, in 1843, but his method of experimenting differed essentially from that employed in these researches, and was not susceptible of any very great degree of accuracy. The relation between the amount of action and the mass of the sensitive gas has not as yet been fully determined; experiment has however already shown that the relation is not a simple one.

Many very interesting phænomena were observed in the course of these investigations. When the gas is first exposed to the light no action whatever is observed; after a short time the absorption slowly begins, and increases until a maximum has been attained, after which it proceeds regularly. This phænomenon of induction probably depends on a peculiar allotropic change which the chlorine must undergo before it is capable of uniting with the hydrogen.

The speaker concluded by expressing his intention of continuing these experiments at Heidelberg, in order exactly to determine the relation which exists between the amount of action and the volume of gas employed; to investigate the phænomenon of induction; and to obtain, if possible, an absolute measure for the chemical rays.

LXIII. *Intelligence and Miscellaneous Articles.*

ON SOME OF THE PRINCIPAL CAUSES OF ATMOSPHERIC ELECTRICITY. BY M. BECQUEREL.

THE causes which constantly furnish the air with an excess of positive and the earth with an excess of negative electricity, excesses which are capable of giving rise to storms and other phænomena under certain conditions, are still unknown, notwithstanding the endeavours of physicists to discover them.

In studying this question some years ago I observed the electrical effects produced in the tissues of plants, and at the contact of these plants with the soil; in this contact the soil is constantly positive and the plant negative, whatever may be the part of the plant put in metallic communication with it. I then indicated this evolution of electricity as one of the causes of the electricity of the atmosphere. In repeating these experiments a year ago, I was struck by the anomalies manifested, in operating on the margin of a river, or in the river itself, or at a certain distance, near the plant, and I was thus led to study the electrical effects produced at the contact of the soil with a fall or stream of water, of which I then understood all the importance. In last October I communicated to the Academy the first results of my experiments, and I have since been constantly occupied with this question, which leads us to one of the principal sources of atmospheric electricity,—a question of a most compli-

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cated nature, from the numerous causes which conduce to the general effect.

The apparatus employed in these researches consists of—1. diaphragms of porous porcelain, or little bags of sail-cloth, each containing a depolarized plate of gold or platinum, surrounded by charcoal of sugar-candy, with a view to rendering the electrical effects constant during a few moments in order to measure them; 2. tangent compasses of great delicacy, adapted for experiments of this nature; 3. atmospheric electrometers destined to the collection of the electricity of vapours formed above the soil or the water; and 4. various accessories,—amongst others conducting wires of copper, gold and platinum covered with gutta percha, &c.

I have said that the electrical effects produced by the contact of the soil and water are complex, for they vary in direction and intensity according to the substances which compose the soil, or which are dissolved in the water; for the production of electrical effects, it is necessary that there should be a heterogeneity between the water of the river and that by which the soil is moistened. When the waters are slightly alkaline they are negative; when they are acid, as is the case with the earth of heaths, they are positive. The well-waters of Paris often present effects of this kind, in consequence of the infiltration of drainage waters, which change in nature from time to time; thus in the course of a month the electrical effects are seen to change in intensity and sign, without any derangement of the apparatus. From this state of things it results that sometimes there are no electrical effects, as is also the case in experimenting with the water of a river and its sandy banks, or the adjacent lands which are washed during inundations. It is necessary to establish permanent observations to follow all the variations to which the actions of contact are subject, and to be on one's guard against the effects of polarization, which are always to be found in operating only for a few moments. Very commonly the polarization is destroyed in the course of twenty-four hours, and the effects of which we are in search may then be observed. In some exceptional cases the electrical current has sufficient intensity to cause the action of a needle telegraph at a distance of several kilometres.

When water evaporates, either from a stream or from the earth, it must necessarily carry off with it an excess of electricity of the same nature possessed by the one or the other, and this becomes diffused in the atmosphere; this electricity may arise not only from the reaction of the water of the river upon that with which the soil is moistened, but also from the decomposition of organic matter. In the latter case the electricity is always positive, whether it arises from the river or from the soil; in the former the two vapours are of contrary signs; the effects are complex.

From the foregoing it will be understood why storms generally take place in summer, at that period of the year when the decomposition of organic matters and evaporation are at their maximum, and also why they are so frequent and so violent under the tropics at the period when the sun approaches the zenith. This is so true, that in those regions there is always a storm bursting at each instant in a locality suitably placed in relation to the sun.

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The phænomena to which I have just referred are so varied, that it is indispensable, before formulating general principles, to multiply experiments in a place serving as a permanent observatory, then in flat countries and amongst mountains, on the margins of rivers and water-courses, and on the sea-shore, in countries like Holland, where there are large alluvial tracts, in salt-marshes, &c. Then, and then only shall we be able to judge of the importance of the subject with which I am occupied, and which is connected with one of the greatest questions in terrestrial physics.—*Comptes Rendus*, April 14, 1856, p. 661.

ON THE BORONATROCALCITE OF SOUTH AMERICA.

BY C. RAMMELSBERG.

This mineral has lately been frequently referred to. It forms larger or smaller roundish lumps, coated with a yellowish-gray earth, and consisting internally of an aggregation of fine silky needles, amongst which yellowish crystals of Glauberite ($NaO, SO^3 + CaO, SO^3$) sometimes occur. In other respects the substance is quite pure and homogeneous.

The powder dissolves with difficulty in boiling water, and the solution has an alkaline reaction. It is soluble in acids, even in the cold. Analyses gave,—

Chloride of sodium.....	3·17	
Sulphate of soda	0·41	
Sulphate of lime.....	0·39	
Boracic acid	41·82	= 43·70
Lime	12·61	13·13
Soda	6·40	6·67
Potash	0·80	0·83
Water.....	34·40	35·67

As the oxygen of the soda (potash) and lime = 1 : 2, and that of the acid is equal to that of the water, and nine times that of the lime, the mineral consists of 1 atom soda, 2 atoms lime, 6 atoms boracic acid, and 18 atoms water, and must be regarded as a compound of 1 atom of bicarbonate of soda, 2 atoms of biborate of lime, and 18 atoms of water.

The formula $NaO, 2BO^3 + 2(CaO, 2BO^3) + 18HO$ requires—

6 atoms boracic acid	= 2617·2	= 45·63
2 atoms lime.....	= 703·3	= 12·26
1 atom soda.....	= 389·7	= 6·79
18 atoms water	= 2025·0	= 35·32
	5735·2	100·00

The properties of this mineral agree with the description given by Hayes; but his analyses led him to the formula $CaO, 2BO^3 + 6HO$, which is that of borocalcite. Earlier analyses by Ulex and Dick gave less simple formulæ.—Poggendorff's *Annalen*, vol. xcvii. p. 302.

ON A COPROLITIC DEPOSIT IN BOHEMIA. BY PROF. REUSS*.

Prof. Reuss of Prague communicated to the Vienna Imperial Academy of Sciences, November 9, 1855, a note on Fossil Excrements of Fishes in the bituminous marl-slate of the Old Red Sand-

* Communicated by Count Marschell.

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stone near Ober-Langenau, Bohemia. These coprolites are richer in organic matter (sometimes as much as 74·03 per cent.) than any other at present known. Treated with æther, they give a rather considerable quantity of a greasy liquid matter, of disagreeable smell. Other coprolites have in the course of time been deprived of a large proportion of their organic matter by decomposition, or by mechanical agents; those, however, under notice have suffered but little alteration in quantity and quality, owing probably to the circumstance that they have been quickly enveloped with inorganic matter, and in this way secured from the action of water and air. Every one of these coprolites is surrounded with a concretion of magnesian limestone, segregated from the imbedding rock. These concretions likewise contain nitrogenous organic matter, sometimes to the amount of 36·56 per cent. This matter may be supposed to have originated from the decomposition of numerous fishes, the remains of which are frequently found preserved in the concretions.

This considerable deposit of organic matter, besides its scientific interest, may acquire a practical importance. It would be perhaps profitable to use these slates for the production of gas, paraffine, &c., and to use for manure the residuum of these operations, containing a notable proportion of potash and phosphoric acid.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1856.

Chiswick.—April 1, 2. Exceedingly fine. 3. Overcast: rain. 4. Densely clouded: fine, with low white clouds. 5. Fine: cloudy. 6. Fine: frosty at night. 7. Fine: cloudy: rain. 8. Rain. 9. Cloudy: rain. 10. Rain. 11. Fine: showery: rain at night. 12. Rain: cloudy and mild: fine. 13. Fine: cloudy: hazy. 14. Fine: rain: boisterous, with rain at night. 15. Overcast: cold north-east wind. 16. Fine, but cold: masses of white clouds. 17. Dusky white clouds: fine: cloudy. 18. Overcast: fine: cloudy. 19. Overcast: densely clouded: clear: frosty. 20. Fine: frosty at night. 21. Cloudless: very fine: hazy at night. 22. Overcast: cloudy: frosty. 23. Slight haze: cloudy. 24. Uniform haze: overcast: fine. 25. Foggy: very fine: rain. 26. Heavy rain: cloudy. 27. Rain. 28. Clear: fine: frosty. 29. Partially overcast: cloudy and cold. 30. Fine.

Mean temperature of the month	46°·48
Mean temperature of April 1855	46 ·08
Mean temperature of April for the last thirty years	47 ·13
Average amount of rain in April	1·553 inch.

Boston.—April 1. Fine: rain P.M. 2. Cloudy. 3. Cloudy: rain P.M. 4. Cloudy. 5. Fine. 6. Cloudy: rain P.M. 7. Cloudy. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain P.M. 11. Fine: rain P.M. 12. Rain A.M. and P.M. 13. Fine. 14. Cloudy. 15. Fine. 16—19. Cloudy. 20. Fine. 21—24. Cloudy. 25. Fine. 26. Rain A.M. and P.M. 27. Cloudy. 28. Cloudy: rain P.M. 29. Cloudy: rain A.M. and P.M. 30. Cloudy.

Sandwick Manse, Orkney.—April 1—3. Bright A.M.: cloudy P.M. 4. Cloudy, drops A.M.: clear, aurora P.M. 5. Cloudy, drops A.M.: clear P.M. 6. Damp A.M.: clear P.M. 7. Bright A.M.: drops P.M. 8—10. Cloudy A.M. and P.M. 11. Showers, cloudy A.M.: clear P.M. 12—14. Cloudy A.M. and P.M. 15. Cloudy A.M.: clear, fine P.M. 16. Cloudy A.M. and P.M. 17. Showers, cloudy A.M.: cloudy P.M. 18. Showers, cloudy A.M.: clear, fine P.M. 19. Clear A.M.: drizzle P.M. 20—22. Cloudy A.M. and P.M. 23. Clear A.M.: cloudy P.M. 24. Cloudy A.M.: cloudy, fine P.M. 25. Cloudy, fine A.M.: cloudy, drops P.M. 26. Clear A.M.: hail-showers P.M. 27. Hail-showers A.M.: sleet-showers P.M. 28. Sleet-showers A.M. and P.M. 29. Sleet-showers A.M.: cloudy P.M. 30. Bright A.M.: cloudy P.M.

Mean temperature of April for previous twenty-nine years	43°·47
Mean temperature of this month	44 ·56
Mean temperature of April 1855	43 ·20
Average quantity of rain in April for fifteen previous years	1·90 inch.

The drought is quite unprecedented, only ·68 of rain having fallen for two months.

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Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1856. April.	Max.	Min.	8½ a.m.	9¼ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	9¼ a.m.	8½ p.m.	1 p.m.					
1.	29·910	29·830	29·63	29·79	29·79	68	33	39	49½	42	s.	sse.	se.	.....	.....	.....
2.	29·793	29·692	29·40	29·73	29·63	66	42	51	48½	43	s.	s.	se.	·01	·02	.....
3.	29·823	29·666	29·37	29·46	29·41	54	44	50	48½	45	sw.	s.	se.	·19	·03	.....
4.	29·773	29·654	29·25	29·29	29·35	59	30	50	47½	39	sw.	s.	ssw.	.....	.....	.....
● 5.	29·598	29·318	29·26	29·30	29·30	57	40	42	47	42	s.	s.	se.	.....	.....	.....
6.	29·330	29·248	28·91	29·34	29·46	57	27	49	45	41	se.	s.	se.	.....	.....	.....
7.	29·493	29·446	29·10	29·41	29·38	59	39	48	44	42½	sw.	w.	se.	·04	·10	.....
8.	29·260	29·183	28·95	29·40	29·44	53	35	49	43	42½	se.	sw.	ese.	·06	.....	.....
9.	29·305	29·273	28·90	29·37	29·33	56	42	48	45½	42	sw.	sw.	e.	·30	·12	.....
10.	29·463	29·143	28·84	29·32	29·39	60	41	48	46	43	sw.	se.	e.	·04	.....	.....
11.	29·654	29·552	29·24	29·48	29·53	62	47	50	46	39½	sw.	w.	ese.	·18	·14	·09
☽ 12.	29·591	29·454	28·98	29·48	29·51	63	33	51	43½	42	sw.	w.	ne.	·01	·14	.....
13.	29·632	29·585	29·24	29·68	29·91	67	36	51	45½	44	s.	sw.	nne.	.....	·15	.....
14.	29·804	29·757	29·44	30·09	30·29	58	42	49	45	42	ne.	ne.	nne.	·16	.....	.....
15.	30·058	29·859	29·66	30·37	30·38	58	37	46	42½	35	ne.	ne.	.....	.....	.....	.....
16.	30·119	30·100	29·87	30·37	30·32	54	31	45	44	45	ne.	ne.	w.	.....	.....	.....
17.	30·139	30·113	29·85	30·27	30·23	55	39	46	48	44½	ne.	ne.	nw.	.....	.....	·02
18.	30·103	30·058	29·77	30·04	30·15	60	31	47	47½	36	ne.	n.	calm	·02	.....	·02
19.	30·238	30·123	29·79	30·12	30·13	49	25	48	44½	46	ne.	n.	w.	.....	.....	.....
○ 20.	30·269	30·209	29·90	30·12	30·12	57	23	42·5	49½	46½	ne.	w.	w.	.....	.....	.....
21.	30·226	30·110	29·85	30·04	30·02	62	27	48	49½	47	ne.	n.	wsw.	.....	.....	.....
22.	30·034	29·967	29·64	29·92	29·82	56	24	55	51	47	ne.	se.	sw.	.....	.....	.....
23.	29·907	29·872	29·50	29·76	29·82	57	37	51·5	53½	47	se.	s.	w.	.....	.....	.....
24.	29·891	29·780	29·50	29·86	29·86	59	35	52	54	47	ne.	se.	calm	.....	.....	.....
25.	29·695	29·591	29·34	29·72	29·68	76	35	52	54	41½	se.	e.	nne.	·22	.....	.....
26.	29·487	29·457	29·00	29·67	29·73	63	40	52·5	43	38	sw.	n.	ne.	·53	·36	.....
☾ 27.	29·560	29·442	29·17	29·71	29·68	44	32	43	43	41	e.	ne.	n.	·14	·36	·13
28.	29·561	29·540	29·22	29·63	29·73	55	25	46	44½	40	n.	w.	nne.	.....	.....	·07
29.	29·610	29·568	29·21	29·76	29·86	56	31	45	42½	40	nw.	nw.	nne.	.....	·06	·01
30.	29·633	29·610	29·28	29·88	29·93	57	29	47	47	40½	nw.	nw.	ne.	·07	·10	.....
Mean.	29·765	29·673	29·36	29·744	29·772	58·56	34·40	48·0	46·75	42·38				1·97	1·58	0·34

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THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XI. FOURTH SERIES.

LXIV. *On the Interaction of Natural Forces**. *By* H. HELMHOLTZ, *Professor of Physiology in the University of Königsberg*†.

THE following article is a translation of a popular lecture, but it will, I doubt not, be studied with interest by many of the readers of this Journal. I once had thoughts of presenting the lecture in a condensed form, omitting allusions, which though proper and necessary in a spoken discourse, might not appear so in a strictly scientific article. On reflection, however, I think it better to let the accomplished author state, in his own fashion, the important question which he has contributed so much to expand and elucidate.—J. T.

A new conquest of very general interest has been recently made by natural philosophy. In the following pages I will endeavour to give a notion of the nature of this conquest. It has reference to a new and universal natural law, which rules the action of natural forces in their mutual relations towards each other, and is as influential on our theoretic views of natural processes as it is important in their technical applications.

Among the practical arts which owe their progress to the development of the natural sciences, from the conclusion of the middle ages downwards, practical mechanics, aided by the mathematical science which bears the same name, was one of the most prominent. The character of the art was, at the time referred to, naturally very different from its present one. Surprised and stimulated by its own success, it thought no problem beyond its power, and immediately attacked some of the most difficult and complicated. Thus it was attempted to build automaton figures which should perform the functions of men and animals. The wonder of the last century was Vaucanson's duck, which fed and digested its food; the flute player of the same

* A Popular Scientific Discourse, delivered the 7th of February, 1854.

† Now of the University of Bonn.

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artist, which moved all its fingers correctly; the writing boy of the older, and the pianoforte player of the younger Droz; which latter, when performing, followed its hands with its eyes, and at the conclusion of the piece bowed courteously to the audience. That men like those mentioned, whose talent might bear comparison with the most inventive heads of the present age, should spend so much time in the construction of these figures which we at present regard as the merest trifles, would be incomprehensible, if they had not hoped in solemn earnest to solve a great problem. The writing boy of the elder Droz was publicly exhibited in Germany some years ago. Its wheelwork is so complicated, that no ordinary head would be sufficient to decipher its manner of action. When, however, we are informed that this boy and its constructor, being suspected of the black art, lay for a time in the Spanish Inquisition, and with difficulty obtained their freedom, we may infer that in those days even such a toy appeared great enough to excite doubts as to its natural origin. And though these artists may not have hoped to breathe into the creature of their ingenuity a soul gifted with moral completeness, still there were many who would be willing to dispense with the moral qualities of their servants, if at the same time their immoral qualities could also be got rid of; and accept, instead of the mutability of flesh and bones, services which should combine the regularity of a machine with the durability of brass and steel. The object, therefore, which the inventive genius of the past century placed before it with the fullest earnestness, and not as a piece of amusement merely, was boldly chosen, and was followed up with an expenditure of sagacity which has contributed not a little to enrich the mechanical experience which a later time knew how to take advantage of. We no longer seek to build machines which shall fulfil the thousand services required of *one* man, but desire, on the contrary, that a machine shall perform *one* service, but shall occupy in doing it the place of a thousand men.

From these efforts to imitate living creatures, another idea, also by a misunderstanding, seems to have developed itself, which, as it were, formed the new philosopher's stone of the seventeenth and eighteenth centuries. It was now the endeavour to construct a perpetual motion. Under this term was understood a machine, which, without being wound up, without consuming in the working of it falling water, wind, or any other natural force, should still continue in motion, the motive power being perpetually supplied by the machine itself. Beasts and human beings seemed to correspond to the idea of such an apparatus, for they moved themselves energetically and incessantly as long as they lived, and were never wound up; nobody set them in motion.

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A connexion between the taking-in of nourishment and the development of force did not make itself apparent. The nourishment seemed only necessary to grease, as it were, the wheelwork of the animal machine, to replace what was used up, and to renew the old. The development of force out of itself seemed to be the essential peculiarity, the real quintessence of organic life. If, therefore, men were to be constructed, a perpetual motion must first be found.

Another hope also seemed to take up incidentally the second place, which in our wiser age would certainly have claimed the first rank in the thoughts of men. The perpetual motion was to produce work inexhaustibly without corresponding consumption, that is to say, out of nothing. Work, however, is money. Here, therefore, the great practical problem which the cunning heads of all centuries have followed in the most diverse ways, namely to fabricate money out of nothing, invited solution. The similarity with the philosopher's stone sought by the ancient chemists was complete. That also was thought to contain the quintessence of organic life, and to be capable of producing gold.

The spur which drove men to inquiry was sharp, and the talent of some of the seekers must not be estimated as small. The nature of the problem was quite calculated to entice poring brains, to lead them round a circle for years, deceiving ever with new expectations which vanished upon nearer approach, and finally reducing these dupes of hope to open insanity. The phantom could not be grasped. It would be impossible to give a history of these efforts, as the clearer heads, among whom the elder Droz must be ranked, convinced themselves of the futility of their experiments, and were naturally not inclined to speak much about them. Bewildered intellects, however, proclaimed often enough that they had discovered the grand secret; and as the incorrectness of their proceedings was always speedily manifest, the matter fell into bad repute, and the opinion strengthened itself more and more that the problem was not capable of solution; one difficulty after another was brought under the dominion of mathematical mechanics, and finally a point was reached where it could be proved, that at least by the use of pure mechanical forces no perpetual motion could be generated.

We have here arrived at the idea of the driving force or power of a machine, and shall have much to do with it in future. I must therefore give an explanation of it. The idea of work is evidently transferred to machines by comparing their arrangements with those of men and animals, to replace which they were applied. We still reckon the work of steam-engines according to horse power. The value of manual labour is determined partly by the force which is expended in it (a strong

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labourer is valued more highly than a weak one), partly, however, by the skill which is brought into action. A machine, on the contrary, which executes work skilfully, can always be multiplied to any extent; hence its skill has not the high value of human skill in domains where the latter cannot be supplied by machines. Thus the idea of the quantity of work in the case of machines has been limited to the consideration of the expenditure of force; this was the more important, as indeed most machines are constructed for the express purpose of exceeding, by the magnitude of their effects, the powers of men and animals. Hence, in a mechanical sense, the idea of work is become identical with that of the expenditure of force, and in this way I will apply it in the following pages.

How, then, can we measure this expenditure, and compare it in the case of different machines?

I must here conduct you a portion of the way—as short a portion as possible—over the uninviting field of mathematico-mechanical ideas, in order to bring you to a point of view from which a more rewarding prospect will open. And though the example which I will here choose, namely that of a water-mill with iron hammer, appears to be tolerably romantic, still, alas, I must leave the dark forest valley, the spark-emitting anvil, and the black Cyclops wholly out of sight, and beg a moment's attention for the less poetic side of the question, namely the machinery. This is driven by a water-wheel, which in its turn is set in motion by the falling water. The axle of the water-wheel has at certain places small projections, thumbs, which, during the rotation, lift the heavy hammer and permit it to fall again. The falling hammer belabours the mass of metal, which is introduced beneath it. The work therefore done by the machine consists, in this case, in the lifting of the hammer, to do which the gravity of the latter must be overcome. The expenditure of force will in the first place, other circumstances being equal, be proportional to the weight of the hammer; it will, for example, be double when the weight of the hammer is doubled. But the action of the hammer depends not upon its weight alone, but also upon the height from which it falls. If it falls through two feet, it will produce a greater effect than if it falls through only one foot. It is, however, clear that if the machine, with a certain expenditure of force, lifts the hammer a foot in height, the same amount of force must be expended to raise it a second foot in height. The work is therefore not only doubled when the weight of the hammer is increased twofold, but also when the space through which it falls is doubled. From this it is easy to see that the work must be measured by the product of the weight into the space through which it ascends. And

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in this way, indeed, do we measure in mechanics. The unit of work is a foot-pound, that is, a pound weight raised to the height of one foot.

While the work in this case consists in the raising of the heavy hammer-head, the driving force which sets the latter in motion is generated by falling water. It is not necessary that the water should fall vertically, it can also flow in a moderately inclined bed; but it must always, where it has water-mills to set in motion, move from a higher to a lower position. Experiment and theory coincide in teaching, that when a hammer of a hundredweight is to be raised one foot, to accomplish this at least a hundredweight of water must fall through the space of one foot; or what is equivalent to this, two hundredweight must fall half a foot, or four hundredweight a quarter of a foot, &c. In short, if we multiply the weight of the falling water by the height through which it falls, and regard, as before, the product as the measure of the work, then the work performed by the machine in raising the hammer can, in the most favourable case, be only equal to the number of foot-pounds of water which have fallen in the same time. In practice, indeed, this ratio is by no means attained: a great portion of the work of the falling water escapes unused, inasmuch as part of the force is willingly sacrificed for the sake of obtaining greater speed.

I will further remark, that this relation remains unchanged whether the hammer is driven immediately by the axle of the wheel, or whether—by the intervention of wheelwork, endless screws, pulleys, ropes,—the motion is transferred to the hammer. We may, indeed, by such arrangements succeed in raising a hammer of ten hundredweight, when by the first simple arrangement the elevation of a hammer one hundredweight might alone be possible; but either this heavier hammer is raised to only one-tenth of the height, or tenfold the time is required to raise it to the same height; so that, however we may alter, by the interposition of machinery, the intensity of the acting force, still in a certain time, during which the mill-stream furnishes us with a definite quantity of water, a certain definite quantity of work, and no more, can be performed.

Our machinery, therefore, has in the first place done nothing more than make use of the gravity of the falling water in order to overpower the gravity of the hammer, and to raise the latter. When it has lifted the hammer to the necessary height, it again liberates it, and the hammer falls upon the metal mass which is pushed beneath it. But why does the falling hammer here exercise a greater force than when it is permitted simply to press with its own weight on the mass of metal? Why is its power greater as the height from which it falls is increased?

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We find, in fact, that the work performed by the hammer is determined by its velocity. In other cases, also, the velocity of moving masses is a means of producing great effects. I only remind you of the destructive effects of musket-bullets, which in a state of rest are the most harmless things in the world. I remind you of the windmill, which derives its force from the moving air. It may appear surprising that motion, which we are accustomed to regard as a non-essential and transitory endowment of bodies, can produce such great effects. But the fact is, that motion appears to us, under ordinary circumstances, transitory, because the movement of all terrestrial bodies is resisted perpetually by other forces, friction, resistance of the air, &c., so that the motion is incessantly weakened and finally neutralized. A body, however, which is opposed by no resisting force, when once set in motion moves onward eternally with undiminished velocity. Thus we know that the planetary bodies have moved without change through space for thousands of years. Only by resisting forces can motion be diminished or destroyed. A moving body, such as the hammer or the musket-ball, when it strikes against another, presses the latter together, or penetrates it, until the sum of the resisting forces which the body struck presents to its pressure, or to the separation of its particles, is sufficiently great to destroy the motion of the hammer or of the bullet. The motion of a mass regarded as taking the place of working force is called the living force (*vis viva*) of the mass. The word "living" has of course here no reference whatever to living beings, but is intended to represent solely the force of the motion as distinguished from the state of unchanged rest—from the gravity of a motionless body, for example, which produces an incessant pressure against the surface which supports it, but does not produce any motion.

In the case before us, therefore, we had first power in the form of a falling mass of water, then in the form of a lifted hammer, and thirdly in the form of the living force of the fallen hammer. We should transform the third form into the second, if we, for example, permitted the hammer to fall upon a highly elastic steel beam strong enough to resist the shock. The hammer would rebound, and in the most favourable case would reach a height equal to that from which it fell, but would never rise higher. In this way its mass would ascend; and at the moment when its highest point has been attained it would represent the same number of raised foot-pounds as before it fell, never a greater number; that is to say, living force can generate the same amount of work as that expended in its production. It is therefore equivalent to this quantity of work.

Our clocks are driven by means of sinking weights, and

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our watches by means of the tension of springs. A weight which lies on the ground, an elastic spring which is without tension, can produce no effects: to obtain such we must first raise the weight or impart tension to the spring, which is accomplished when we wind up our clocks and watches. The man who winds the clock or watch communicates to the weight or to the spring a certain amount of power, and exactly so much as is thus communicated is gradually given out again during the following twenty-four hours, the original force being thus slowly consumed to overcome the friction of the wheels and the resistance which the pendulum encounters from the air. The wheelwork of the clock therefore exhibits no working force which was not previously communicated to it, but simply distributes the force given to it uniformly over a longer time.

Into the chamber of an air-gun we squeeze, by means of a condensing air-pump, a great quantity of air. When we afterwards open the cock of the gun and admit the compressed air into the barrel, the ball is driven out of the latter with a force similar to that exerted by ignited powder. Now we may determine the work consumed in the pumping-in of the air, and the living force which, upon firing, is communicated to the ball, but we shall never find the latter greater than the former. The compressed air has generated no working force, but simply gives to the bullet that which has been previously communicated to it. And while we have pumped for perhaps a quarter of an hour to charge the gun, the force is expended in a few seconds when the bullet is discharged; but because the action is compressed into so short a time, a much greater velocity is imparted to the ball than would be possible to communicate to it by the unaided effort of the arm in throwing it.

From these examples you observe, and the mathematical theory has corroborated this for all purely mechanical, that is to say, for moving forces, that all our machinery and apparatus generate no force, but simply yield up the power communicated to them by natural forces,—falling water, moving wind, or by the muscles of men and animals. After this law had been established by the great mathematicians of the last century, a perpetual motion, which should only make use of pure mechanical forces, such as gravity, elasticity, pressure of liquids and gases, could only be sought after by bewildered and ill-instructed people. But there are still other natural forces which are not reckoned among the purely moving forces,—heat, electricity, magnetism, light, chemical forces, all of which nevertheless stand in manifold relation to mechanical processes. There is hardly a natural process to be found which is not accompanied by mechanical actions, or from which mechanical work may not be derived.

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Here the question of a perpetual motion remained open; the decision of this question marks the progress of modern physics, regarding which I promised to address you.

In the case of the air-gun, the work to be accomplished in the propulsion of the ball was given by the arm of the man who pumped in the air. In ordinary firearms, the condensed mass of air which propels the bullet is obtained in a totally different manner, namely by the combustion of the powder. Gunpowder is transformed by combustion for the most part into gaseous products, which endeavour to occupy a much greater space than that previously taken up by the volume of the powder. Thus you see, that, by the use of gunpowder, the work which the human arm must accomplish in the case of the air-gun is spared.

In the mightiest of our machines, the steam-engine, it is a strongly compressed aëriform body, water vapour, which, by its effort to expand, sets the machine in motion. Here also we do not condense the steam by means of an external mechanical force, but by communicating heat to a mass of water in a closed boiler, we change this water into steam, which, in consequence of the limits of the space, is developed under strong pressure. In this case, therefore, it is the heat communicated which generates the mechanical force. The heat thus necessary for the machine we might obtain in many ways: the ordinary method is to procure it from the combustion of coal.

Combustion is a chemical process. A particular constituent of our atmosphere, oxygen, possesses a strong force of attraction, or, as it is named in chemistry, a strong affinity for the constituents of the combustible body, which affinity, however, in most cases can only exert itself at high temperatures. As soon as a portion of the combustible body, for example the coal, is sufficiently heated, the carbon unites itself with great violence to the oxygen of the atmosphere and forms a peculiar gas, carbonic acid, the same which we see foaming from beer and champagne. By this combination light and heat are generated: heat is generally developed by any combination of two bodies of strong affinity for each other; and when the heat is intense enough, light appears. Hence in the steam-engine it is chemical processes and chemical forces which produce the astonishing work of these machines. In like manner the combustion of gunpowder is a chemical process, which in the barrel of the gun communicates living force to the bullet.

While now the steam-engine developes for us mechanical work out of heat, we can conversely generate heat by mechanical forces. A skilful blacksmith can render an iron wedge red-hot by hammering. The axles of our carriages must be protected

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by careful greasing from ignition through friction. Even lately this property has been applied on a large scale. In some factories, where a surplus of water power is at hand, this surplus is applied to cause a strong iron plate to rotate swiftly upon another, so that they become strongly heated by the friction. The heat so obtained warms the room, and thus a stove without fuel is provided. Now could not the heat generated by the plates be applied to a small steam-engine, which in its turn should be able to keep the rubbing plates in motion? The perpetual motion would thus be at length found. This question might be asked, and could not be decided by the older mathematico-mechanical investigations. I will remark beforehand, that the general law which I will lay before you answers the question in the negative.

By a similar plan, however, a speculative American set some time ago the industrial world of Europe in excitement. The magneto-electric machines often made use of in the case of rheumatic disorders are well known to the public. By imparting a swift rotation to the magnet of such a machine we obtain powerful currents of electricity. If those be conducted through water, the latter will be reduced into its two components, oxygen and hydrogen. By the combustion of hydrogen, water is again generated. If this combustion takes place, not in atmospheric air, of which oxygen only constitutes a fifth part, but in pure oxygen, and if a bit of chalk be placed in the flame, the chalk will be raised to a white heat, and give us the sun-like Drummond's light. At the same time the flame developes a considerable quantity of heat. Our American proposed to utilize in this way the gases obtained from electrolytic decomposition, and asserted, that by the combustion a sufficient amount of heat was generated to keep a small steam-engine in action, which again drove his magneto-electric machine, decomposed the water, and thus continually prepared its own fuel. This would certainly have been the most splendid of all discoveries; a perpetual motion which, besides the force that kept it going, generated light like the sun, and warmed all around it. The matter was by no means badly cogitated. Each practical step in the affair was known to be possible; but those which at that time were acquainted with the physical investigations which bear upon this subject, could have affirmed, on first hearing the report, that the matter was to be numbered among the numerous stories of the fable-rich America; and indeed a fable it remained.

It is not necessary to multiply examples further. You will infer from those given in what immediate connexion heat, electricity, magnetism, light, and chemical affinity, stand with mechanical forces.

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Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations*.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, to excite chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might be easily and completely stated. It was found that all known relations of forces harmonize with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

* "Wo ein Tritt tausend Fäden regt,
Die Schifflein herüber hinüber schiessen,
Die Fäden ungesehen fliessen,
Ein Schlag tausend Verbindungen schlägt."

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His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections*.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of view. My inquiry was made public in 1847, in a small pamphlet bearing the title, "On the Conservation of Force†."

Since that time the interest of the scientific public for this subject has gradually augmented, particularly in England, of which I had an opportunity of convincing myself during a visit last summer. A great number of the essential consequences of the above manner of viewing the subject, the proof of which was

* The following extract is taken from a lecture by Mr. Grove, delivered at the London Institution on the 19th of January, 1842:—

"Light, heat, electricity, magnetism, motion, and chemical affinity, are all convertible material affections; assuming any one as the cause, one of the others will be the effect. Thus heat may be said to produce electricity, electricity to produce heat; magnetism to produce electricity, electricity magnetism; and so of the rest. Cause and effect, therefore, in their relation to such forces, are words solely of convenience: we are totally unacquainted with the generating power of each and all of them, and probably shall ever remain so: we can only ascertain the normal of their action: we must humbly refer their causation to one omnipresent influence, and content ourselves with studying their effects, and developing by experiment their mutual relations."

"I have long held an opinion," says Mr. Faraday in 1845, "almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have a common origin, or in other words, are so directly related and mutually dependent, that they are convertible one into another."—Tr.

† A translation of this important essay appears in the Scientific Memoirs, New Series, p. 114.—J. T.

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wanting when the first theoretic notions were published, have since been confirmed by experiment, particularly by those of Joule; and during the last year the most eminent physicist of France, Regnault, has adopted the new mode of regarding the question, and by fresh investigations on the specific heat of gases has contributed much to its support. For some important consequences the experimental proof is still wanting, but the number of confirmations is so predominant, that I have not deemed it too early to bring the subject before even a non-scientific audience.

How the question has been decided you may already infer from what has been stated. In the series of natural processes there is no circuit to be found, by which mechanical force can be gained without a corresponding consumption. The perpetual motion remains impossible. Our reflections, however, gain thereby a higher interest.

We have thus far regarded the development of force by natural processes, only in its relation to its usefulness to man, as mechanical force. You now see that we have arrived at a general law, which holds good wholly independent of the application which man makes of natural forces; we must therefore make the expression of our law correspond to this more general significance. It is in the first place clear, that the work which, by any natural process whatever, is performed under favourable conditions by a machine, and which may be measured in the way already indicated, may be used as a measure of force common to all. Further, the important question arises, If the quantity of force cannot be augmented except by corresponding consumption, can it be diminished or lost? For the purposes of our machines it certainly can, if we neglect the opportunity to convert natural processes to use, but as investigation has proved, not for nature as a whole.

In the collision and friction of bodies against each other, the mechanics of former years assumed simply that living force was lost. But I have already stated that each collision and each act of friction generates heat; and, moreover, Joule has established by experiment the important law, that for every foot-pound of force which is lost a definite quantity of heat is always generated, and that when work is performed by the consumption of heat, for each foot-pound thus gained a definite quantity of heat disappears. The quantity of heat necessary to raise the temperature of a pound of water a degree of the Centigrade thermometer, corresponds to a mechanical force by which a pound weight would be raised to the height of 1350 feet: we name this quantity the mechanical equivalent of heat. I may mention here that these facts conduct of necessity to the conclusion, that heat

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is not, as was formerly imagined, a fine imponderable substance, but that, like light, it is a peculiar shivering motion of the ultimate particles of bodies. In collision and friction, according to this manner of viewing the subject, the motion of the mass of a body which is apparently lost is converted into a motion of the ultimate particles of the body; and conversely, when mechanical force is generated by heat, the motion of the ultimate particles is converted into a motion of the mass.

Chemical combinations generate heat, and the quantity of this heat is totally independent of the time and steps through which the combination has been effected, provided that other actions are not at the same time brought into play. If, however, mechanical work is at the same time accomplished, as in the case of the steam-engine, we obtain as much less heat as is equivalent to this work. The quantity of work produced by chemical force is in general very great. A pound of the purest coal gives, when burnt, sufficient heat to raise the temperature of 8086 pounds of water one degree of the Centigrade thermometer; from this we can calculate that the magnitude of the chemical force of attraction between the particles of a pound of coal and the quantity of oxygen that corresponds to it, is capable of lifting a weight of 100 pounds to a height of twenty miles. Unfortunately in our steam-engines we have hitherto been able to gain only the smallest portion of this work, the greater part is lost in the shape of heat. The best expansive engines give back as mechanical work only 18 per cent. of the heat generated by the fuel.

From a similar investigation of all the other known physical and chemical processes, we arrive at the conclusion that Nature as a whole possesses a store of force which cannot in any way be either increased or diminished, and that therefore the quantity of force in nature is just as eternal and unalterable as the quantity of matter. Expressed in this form, I have named the general law "The Principle of the Conservation of Force."

We cannot create mechanical force, but we may help ourselves from the general storehouse of Nature. The brook and the wind, which drive our mills, the forest and the coal-bed, which supply our steam-engines and warm our rooms, are to us the bearers of a small portion of the great natural supply which we draw upon for our purposes, and the actions of which we can apply as we think fit. The possessor of a mill claims the gravity of the descending rivulet, or the living force of the moving wind, as his possession. These portions of the store of Nature are what give his property its chief value.

Further, from the fact that no portion of force can be absolutely lost, it does not follow that a portion may not be inap-

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plicable to human purposes. In this respect the inferences drawn by William Thomson from the law of Carnot are of importance. This law, which was discovered by Carnot during his endeavours to ascertain the relations between heat and mechanical force, which, however, by no means belongs to the necessary consequences of the conservation of force, and which Clausius was the first to modify in such a manner that it no longer contradicted the above general law, expresses a certain relation between the compressibility, the capacity for heat, and the expansion by heat of all bodies. It is not yet considered as actually proved, but some remarkable deductions having been drawn from it, and afterwards proved to be facts by experiment, it has attained thereby a great degree of probability. Besides the mathematical form in which the law was first expressed by Carnot, we can give it the following more general expression:—"Only when heat passes from a warmer to a colder body, and even then only partially, can it be converted into mechanical work."

The heat of a body which we cannot cool further, cannot be changed into another form of force; into the electric or chemical force, for example. Thus in our steam-engines we convert a portion of the heat of the glowing coal into work, by permitting it to pass to the less warm water of the boiler. If, however, all the bodies in nature had the same temperature, it would be impossible to convert any portion of their heat into mechanical work. According to this we can divide the total force store of the universe into two parts, one of which is heat, and must continue to be such; the other, to which a portion of the heat of the warmer bodies, and the total supply of chemical, mechanical, electrical, and magnetical forces belong, is capable of the most varied changes of form, and constitutes the whole wealth of change which takes place in nature.

But the heat of the warmer bodies strives perpetually to pass to bodies less warm by radiation and conduction, and thus to establish an equilibrium of temperature. At each motion of a terrestrial body a portion of mechanical force passes by friction or collision into heat, of which only a part can be converted back again into mechanical force. This is also generally the case in every electrical and chemical process. From this it follows that the first portion of the store of force, the unchangeable heat, is augmented by every natural process, while the second portion, mechanical, electrical, and chemical force, must be diminished; so that if the universe be delivered over to the undisturbed action of its physical processes, all force will finally pass into the form of heat, and all heat come into a state of equilibrium. Then all possibility of a further change would be

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at an end, and the complete cessation of all natural processes must set in. The life of men, animals, and plants, could not of course continue if the sun had lost his high temperature, and with it his light,—if all the components of the earth's surface had closed those combinations which their affinities demand. In short, the universe from that time forward would be condemned to a state of eternal rest.

These consequences of the law of Carnot are of course only valid, provided that the law, when sufficiently tested, proves to be universally correct. In the mean time there is little prospect of the law being proved incorrect. At all events we must admire the sagacity of Thomson, who, in the letters of a long-known little mathematical formula, which only speaks of the heat, volume and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death.

I have already given you notice that our path lay through a thorny and unrefreshing field of mathematico-mechanical developments. We have now left this portion of our road behind us. The general principle which I have sought to lay before you has conducted us to a point from which our view is a wide one, and aided by this principle, we can now at pleasure regard this or the other side of the surrounding world, according as our interest in the matter leads us. A glance into the narrow laboratory of the physicist, with its small appliances and complicated abstractions, will not be so attractive as a glance at the wide heaven above us, the clouds, the rivers, the woods, and the living beings around us. While regarding the laws which have been deduced from the physical processes of terrestrial bodies as applicable also to the heavenly bodies, let me remind you that the same force which, acting at the earth's surface, we call gravity (*Schwere*), acts as gravitation in the celestial spaces, and also manifests its power in the motion of the immeasurably distant double stars which are governed by exactly the same laws as those subsisting between the earth and moon; that therefore the light and heat of terrestrial bodies do not in any way differ essentially from those of the sun, or of the most distant fixed star; that the meteoric stones which sometimes fall from external space upon the earth are composed of exactly the same simple chemical substances as those with which we are acquainted. We need therefore feel no scruple in granting that general laws to which all terrestrial natural processes are subject, are also valid for other bodies than the earth. We will therefore make use of our law to glance over the household of the universe with respect to the store of force, capable of action, which it possesses.

A number of singular peculiarities in the structure of our

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planetary system, indicate that it was once a connected mass with a uniform motion of rotation. Without such an assumption it is impossible to explain why all the planets move in the same direction round the sun, why they all rotate in the same direction round their axes, why the planes of their orbits, and those of their satellites and rings, all nearly coincide, why all their orbits differ but little from circles, and much besides. From these remaining indications of a former state astronomers have shaped an hypothesis regarding the formation of our planetary system, which although from the nature of the case it must ever remain an hypothesis, still in its special traits is so well supported by analogy, that it certainly deserves our attention, and the more so, as this notion in our own home, and within the walls of this town*, first found utterance. It was Kant who, feeling great interest in the physical description of the earth and the planetary system, undertook the labour of studying the works of Newton, and as an evidence of the depth to which he had penetrated into the fundamental ideas of Newton, seized the notion that the same attractive force of all ponderable matter which now supports the motion of the planets, must also aforetime have been able to form from matter loosely scattered in space the planetary system. Afterwards, and independent of Kant, Laplace, the great author of the *Mécanique Céleste*, laid hold of the same thought, and introduced it among astronomers.

The commencement of our planetary system, including the sun, must, according to this, be regarded as an immense nebulous mass which filled the portion of space which is now occupied by our system, far beyond the limits of Neptune, our most distant planet. Even now we perhaps see similar masses in the distant regions of the firmament, as patches of nebulæ, and nebulous stars; within our system also, comets, the zodiacal light, the corona of the sun during a total eclipse, exhibit remnants of a nebulous substance, which is so thin that the light of the stars passes through it unenfeebled and unrefracted. If we calculate the density of the mass of our planetary system, according to the above assumption, for the time when it was a nebulous sphere, which reached to the path of the outmost planet, we should find that it would require several cubic miles of such matter to weigh a single grain.

The general attractive force of all matter must, however, impel these masses to approach each other, and to condense, so that the nebulous sphere became incessantly smaller, by which, according to mechanical laws, a motion of rotation originally slow, and the existence of which must be assumed, would gradually become quicker and quicker. By the centrifugal force which

* Königsberg.

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must act most energetically in the neighbourhood of the equator of the nebulous sphere, masses could from time to time be torn away, which afterwards would continue their courses separate from the main mass, forming themselves into single planets, or, similar to the great original sphere, into planets with satellites and rings, until finally the principal mass condensed itself into the sun. With regard to the origin of heat and light this view gives us no information.

When the nebulous chaos first separated itself from other fixed star masses, it must not only have contained all kinds of matter which was to constitute the future planetary system, but also, in accordance with our new law, the whole store of force which at one time must unfold therein its wealth of actions. Indeed in this respect an immense dower was bestowed in the shape of the general attraction of all the particles for each other. This force, which on the earth exerts itself as gravity, acts in the heavenly spaces as gravitation. As terrestrial gravity when it draws a weight downwards performs work and generates *vis viva*, so also the heavenly bodies do the same when they draw two portions of matter from distant regions of space towards each other.

The chemical forces must have been also present, ready to act; but as these forces can only come into operation by the most intimate contact of the different masses, condensation must have taken place before the play of chemical forces began.

Whether a still further supply of force in the shape of heat was present at the commencement we do not know. At all events, by aid of the law of the equivalence of heat and work, we find in the mechanical forces existing at the time to which we refer, such a rich source of heat and light, that there is no necessity whatever to take refuge in the idea of a store of these forces originally existing*. When through condensation of the masses their particles came into collision and clung to each other, the *vis viva* of their motion would be thereby annihilated, and must reappear as heat. Already in old theories it has been calculated that cosmical masses must generate heat by their collision, but it was far from anybody's thought to make even a guess at the amount of heat to be generated in this way. At present we can give definite numerical values with certainty.

Let us make this addition to our assumption; that, at the commencement, the density of the nebulous matter was a vanishing quantity as compared with the present density of the sun and planets; we can then calculate how much work has been performed by the condensation; we can further calculate how much of this work still exists in the form of mechanical force, as

* No necessity for a "Firemist."—Tr.

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attraction of the planets towards the sun, and as *vis viva* of their motion, and find by this how much of the force has been converted into heat.

The result of this calculation * is, that only about the 454th part of the original mechanical force remains as such, and that the remainder, converted into heat, would be sufficient to raise a mass of water equal to the sun and planets taken together, not less than 28 millions of degrees of the Centigrade scale. For the sake of comparison, I will mention that the highest temperature which we can produce by the oxyhydrogen blowpipe, which is sufficient to fuse and vaporize even platina, and which but few bodies can endure, is estimated at about 2000 degrees. Of the action of a temperature of 28 millions of such degrees we can form no notion. If the mass of our entire system were pure coal, by the combustion of the whole of it only the 3500th part of the above quantity would be generated. This is also clear, that such a great development of heat must have presented the greatest obstacle to the speedy union of the masses, that the greater part of the heat must have been diffused by radiation into space, before the masses could form bodies possessing the present density of the sun and planets, and that these bodies must once have been in a state of fiery fluidity. This notion is corroborated by the geological phænomena of our planet; and with regard to the other planetary bodies, the flattened form of the sphere, which is the form of equilibrium of a fluid mass, is indicative of a former state of fluidity. If I thus permit an immense quantity of heat to disappear without compensation from our system, the principle of the conservation of force is not thereby invaded. Certainly for our planet it is lost, but not for the universe. It has proceeded outwards, and daily proceeds outwards into infinite space; and we know not whether the medium which transmits the undulations of light and heat, possesses an end where the rays must return, or whether they eternally pursue their way through infinitude.

The store of force at present possessed by our system is also equivalent to immense quantities of heat. If our earth were by a sudden shock brought to rest in her orbit,—which is not to be feared in the existing arrangement of our system—by such a shock a quantity of heat would be generated equal to that produced by the combustion of fourteen such earths of solid coal. Making the most unfavourable assumption as to its capacity for heat, that is, placing it equal to that of water, the mass of the earth would thereby be heated 11200 degrees; it would therefore be quite fused and for the most part reduced to vapour. If then

* See note at the end.

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the earth, after having been thus brought to rest, should fall into the sun, which of course would be the case, the quantity of heat developed by the shock would be 400 times greater.

Even now from time to time such a process is repeated on a small scale. There can hardly be a doubt that meteors, fireballs, and meteoric stones are masses which belong to the universe, and before coming into the domain of our earth, moved like the planets round the sun. Only when they enter our atmosphere do they become visible and fall sometimes to the earth. In order to explain the emission of light by these bodies, and the fact that for some time after their descent they are very hot, the friction was long ago thought of which they experience in passing through the air. We can now calculate that a velocity of 8000 feet a second, supposing the whole of the friction to be expended in heating the solid mass, would raise a piece of meteoric iron 1000° C. in temperature, or, in other words, to a vivid red heat. Now the average velocity of the meteors seems to be thirty or forty times the above amount. To compensate this, however, the greater portion of the heat is doubtless carried away by the condensed mass of air which the meteor drives before it. It is known that bright meteors generally leave a luminous trail behind them, which probably consists of severed portions of the red-hot surfaces. Meteoric masses which fall to the earth often burst with a violent explosion, which may be regarded as a result of the quick heating. The newly-fallen pieces have been for the most part found hot, but not red-hot, which is easily explainable by the circumstance, that during the short time occupied by the meteor in passing through the atmosphere, only a thin superficial layer is heated to redness, while but a small quantity of heat has been able to penetrate to the interior of the mass. For this reason the red heat can speedily disappear.

Thus has the falling of the meteoric stone, the minute remnant of processes which seem to have played an important part in the formation of the heavenly bodies, conducted us to the present time, where we pass from the darkness of hypothetical views to the brightness of knowledge. In what we have said, however, all that is hypothetical is the assumption of Kant and Laplace, that the masses of our system were once distributed as nebulæ in space.

On account of the rarity of the case, we will still further remark in what close coincidence the results of science here stand with the earlier legends of the human family, and the forebodings of poetic fancy. The cosmogony of ancient nations generally commences with chaos and darkness.

Neither is the Mosaic tradition very divergent, particularly when we remember that that which Moses names heaven, is dif-

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ferent from the blue dome above us, and is synonymous with space, and that the unformed earth and the waters of the great deep, which were afterwards divided into waters above the firmament and waters below the firmament, resembled the chaotic components of the world.

Our earth bears still the unmistakeable traces of its old fiery fluid condition. The granite formations of her mountains exhibit a structure, which can only be produced by the crystallization of fused masses. Investigation still shows that the temperature in mines and borings increases as we descend; and if this increase is uniform, at the depth of fifty miles a heat exists sufficient to fuse all our minerals*. Even now our volcanoes project from time to time mighty masses of fused rocks from their interior, as a testimony of the heat which exists there. But the cooled crust of the earth has already become so thick, that, as may be shown by calculations of its conductive power, the heat coming to the surface from within, in comparison with that reaching the earth from the sun, is exceedingly small, and increases the temperature of the surface only about $\frac{1}{30}$th of a degree Centigrade; so that the remnant of the old store of force which is enclosed as heat within the bowels of the earth, has a sensible influence upon the processes at the earth's surface only through the instrumentality of volcanic phænomena. These processes owe their power almost wholly to the action of other heavenly bodies, particularly to the light and heat of the sun, and partly also, in the case of the tides, to the attraction of the sun and moon.

Most varied and numerous are the changes which we owe to the light and heat of the sun. The sun heats our atmosphere irregularly, the warm rarefied air ascends, while fresh cool air flows from the sides to supply its place: in this way winds are generated. This action is most powerful at the equator, the warm air of which incessantly flows in the upper regions of the atmosphere towards the poles; while just as persistently at the earth's surface, the trade-wind carries new and cool air to the equator. Without the heat of the sun, all winds must of necessity cease. Similar currents are produced by the same cause in the waters of the sea. Their power may be inferred from the influence which in some cases they exert upon climate. By them the warm water of the Antilles is carried to the British Isles, and confers upon them a mild uniform warmth, and rich moisture; while, through similar causes, the floating ice of the North Pole is

* This is not probable. The greater density and consequent better conductivity of the mass, and the elevation of the point of fusion by pressure, established by the researches of Messrs. Hopkins and Fairbairn, would throw the region of liquidity deeper.—Tr.

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carried to the coast of Newfoundland and produces raw cold. Further, by the heat of the sun a portion of the water is converted into vapour, which rises in the atmosphere, is condensed to clouds, or falls in rain and snow upon the earth, collects in the form of springs, brooks and rivers, and finally reaches the sea again, after having gnawed the rocks, carried away light earth, and thus performed its part in the geologic changes of the earth; perhaps besides all this it has driven our water-mill upon its way. If the heat of the sun were withdrawn, there would remain only a single motion of water, namely the tides, which are produced by the attraction of the sun and moon.

How is it, now, with the motions and the work of organic beings? To the builders of the automata of the last century, men and animals appeared as clockwork which was never wound up, and created the force which they exerted out of nothing. They did not know how to establish a connexion between the nutriment consumed and the work generated. Since, however, we have learned to discern in the steam-engine this origin of mechanical force, we must inquire whether something similar does not hold good with regard to men. Indeed, the continuation of life is dependent on the consumption of nutritive materials: these are combustible substances, which, after digestion and being passed into the blood, actually undergo a slow combustion, and finally enter into almost the same combinations with the oxygen of the atmosphere that are produced in an open fire. As the quantity of heat generated by combustion is independent of the duration of the combustion and the steps in which it occurs, we can calculate from the mass of the consumed material how much heat, or its equivalent work, is thereby generated in an animal body. Unfortunately, the difficulty of the experiments is still very great; but within those limits of accuracy which have been as yet attainable, the experiments show that the heat generated in the animal body corresponds to the amount which would be generated by the chemical processes. The animal body therefore does not differ from the steam-engine as regards the manner in which it obtains heat and force, but does differ from it in the manner in which the force gained is to be made use of. The body is, besides, more limited than the machine in the choice of its fuel; the latter could be heated with sugar, with starch-flour, and butter, just as well as with coal or wood; the animal body must dissolve its materials artificially, and distribute them through its system; it must, further, perpetually renew the used-up materials of its organs, and as it cannot itself create the matter necessary for this, the matter must come from without. Liebig was the first to point out these various uses of the consumed nutriment. As material for the perpetual renewal of the

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body, it seems that certain definite albuminous substances which appear in plants, and form the chief mass of the animal body, can alone be used. They form only a portion of the mass of nutriment taken daily; the remainder, sugar, starch, fat, are really only materials for warming, and are perhaps not to be superseded by coal, simply because the latter does not permit itself to be dissolved.

If, then, the processes in the animal body are not in this respect to be distinguished from inorganic processes, the question arises, whence comes the nutriment which constitutes the source of the body's force? The answer is, from the vegetable kingdom; for only the material of plants, or the flesh of plant-eating animals, can be made use of for food. The animals which live on plants occupy a mean position between carnivorous animals, in which we reckon man, and vegetables, which the former could not make use of immediately as nutriment. In hay and grass the same nutritive substances are present as in meal and flour, but in less quantity. As, however, the digestive organs of man are not in a condition to extract the small quantity of the useful from the great excess of the insoluble, we submit, in the first place, these substances to the powerful digestion of the ox, permit the nourishment to store itself in the animal's body, in order in the end to gain it for ourselves in a more agreeable and useful form. In answer to our question, therefore, we are referred to the vegetable world. Now when what plants take in and what they give out are made the subjects of investigation, we find that the principal part of the former consists in the products of combustion which are generated by the animal. They take the consumed carbon given off in respiration, as carbonic acid, from the air, the consumed hydrogen as water, the nitrogen in its simplest and closest combination as ammonia; and from these materials, with the assistance of small ingredients which they take from the soil, they generate anew the compound combustible substances, albumen, sugar, oil, on which the animal subsists. Here, therefore, is a circuit which appears to be a perpetual store of force. Plants prepare fuel and nutriment, animals consume these, burn them slowly in their lungs, and from the products of combustion the plants again derive their nutriment. The latter is an eternal source of chemical, the former of mechanical forces. Would not the combination of both organic kingdoms produce the perpetual motion? We must not conclude hastily: further inquiry shows, that plants are capable of producing combustible substances only when they are under the influence of the sun. A portion of the sun's rays exhibits a remarkable relation to chemical forces,—it can produce and destroy chemical combinations; and these rays, which for the most part are blue or violet,

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are called therefore chemical rays. We make use of their action in the production of photographs. Here compounds of silver are decomposed at the place where the sun's rays strike them. The same rays overpower in the green leaves of plants the strong chemical affinity of the carbon of the carbonic acid for oxygen, give back the latter free to the atmosphere, and accumulate the other, in combination with other bodies, as woody fibre, starch, oil, or resin. These chemically active rays of the sun disappear completely as soon as they encounter the green portions of the plants, and hence it is that in Daguerreotype images the green leaves of plants appear uniformly black. Inasmuch as the light coming from them does not contain the chemical rays, it is unable to act upon the silver compounds.

Hence a certain portion of force disappears from the sunlight, while combustible substances are generated and accumulated in plants; and we can assume it as very probable, that the former is the cause of the latter. I must indeed remark, that we are in possession of no experiments from which we might determine whether the *vis viva* of the sun's rays which have disappeared corresponds to the chemical forces accumulated during the same time; and as long as these experiments are wanting, we cannot regard the stated relation as a certainty. If this view should prove correct, we derive from it the flattering result, that all force, by means of which our bodies live and move, finds its source in the purest sunlight; and hence we are all, in point of nobility, not behind the race of the great monarch of China, who heretofore alone called himself son of the sun. But it must also be conceded, that our lower fellow-beings, the frog and leech, share the same æthereal origin, as also the whole vegetable world, and even the fuel which comes to us from the ages past, as well as the youngest offspring of the forest with which we heat our stoves and set our machines in motion.

You see, then, that the immense wealth of ever-changing meteorological, climatic, geological, and organic processes of our earth are almost wholly preserved in action by the light- and heat-giving rays of the sun; and you see in this a remarkable example, how Proteus-like the effects of a single cause, under altered external conditions, may exhibit itself in nature. Besides these, the earth experiences an action of another kind from its central luminary, as well as from its satellite the moon, which exhibits itself in the remarkable phænomenon of the ebb and flow of the tide.

Each of these bodies excites, by its attraction upon the waters of the sea, two gigantic waves, which flow in the same direction round the world, as the attracting bodies themselves apparently do. The two waves of the moon, on account of her greater nearness, are about $3\frac{1}{2}$ times as large as those excited by the sun.

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One of these waves has its crest on the quarter of the earth's surface which is turned towards the moon, the other is at the opposite side. Both these quarters possess the flow of the tide, while the regions which lie between have the ebb. Although in the open sea the height of the tide amounts to only about three feet, and only in certain narrow channels, where the moving water is squeezed together, rises to thirty feet, the might of the phænomenon is nevertheless manifest from the calculation of Bessel, according to which a quarter of the earth covered by the sea possesses, during the flow of the tide, about 25,000 cubic miles of water more than during the ebb, and that therefore such a mass of water must, in $6\frac{1}{4}$ hours, flow from one quarter of the earth to the other.

The phænomenon of the ebb and flow, as already recognized by Mayer, combined with the law of the conservation of force, stands in remarkable connexion with the question of the stability of our planetary system. The mechanical theory of the planetary motions discovered by Newton teaches, that if a solid body in absolute *vacuo*, attracted by the sun, move around him in the same manner as the planets, this motion will endure unchanged through all eternity.

Now we have actually not only one, but several such planets, which move around the sun, and by their mutual attraction create little changes and disturbances in each other's paths. Nevertheless Laplace, in his great work, the *Mécanique Céleste*, has proved that in our planetary system all these disturbances increase and diminish periodically, and can never exceed certain limits, so that by this cause the eternal existence of the planetary system is unendangered.

But I have already named two assumptions which must be made: first, that the celestial spaces must be absolutely empty; and secondly, that the sun and planets must be solid bodies. The first is at least the case as far as astronomical observations reach, for they have never been able to detect any retardation of the planets, such as would occur if they moved in a resisting medium. But on a body of less mass, the comet of Encke, changes are observed of such a nature: this comet describes ellipses round the sun which are becoming gradually smaller. If this kind of motion, which certainly corresponds to that through a resisting medium, be actually due to the existence of such a medium, a time will come when the comet will strike the sun; and a similar end threatens all the planets, although after a time, the length of which baffles our imagination to conceive of it. But even should the existence of a resisting medium appear doubtful to us, there is no doubt that the planets are not wholly composed of solid materials which are inseparably bound

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together. Signs of the existence of an atmosphere are observed on the Sun, on Venus, Mars, Jupiter, and Saturn. Signs of water and ice upon Mars; and our earth has undoubtedly a fluid portion on its surface, and perhaps a still greater portion of fluid within it. The motions of the tides, however, produce friction, all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion, that every tide, although with infinite slowness, still with certainty diminishes the store of mechanical force of the system; and as a consequence of this, the rotation of the planets in question round their axes must become more slow, they must therefore approach the sun, or their satellites must approach them. What length of time must pass before the length of our day is diminished one second by the action of the tides cannot be calculated, until the height and time of the tide in all portions of the ocean are known. This alteration, however, takes place with extreme slowness, as is known by the consequences which Laplace has deduced from the observations of Hipparchus, according to which, during a period of 2000 years, the duration of the day has not been shortened by the $\frac{1}{300}$dth part of a second. The final consequence would be, but after millions of years, if in the mean time the ocean did not become frozen, that one side of the earth would be constantly turned towards the sun, and enjoy a perpetual day, whereas the opposite side would be involved in eternal night. Such a position we observe in our moon with regard to the earth, and also in the case of the satellites as regards their planets; it is, perhaps, due to the action of the mighty ebb and flow to which these bodies, in the time of their fiery fluid condition, were subjected.

I would not have brought forward these conclusions, which again plunge us in the most distant future, if they were not unavoidable. Physico-mechanical laws are, as it were, the telescopes of our spiritual eye, which can penetrate into the deepest night of time, past and to come.

Another essential question as regards the future of our planetary system has reference to its future temperature and illumination. As the internal heat of the earth has but little influence on the temperature of the surface, the heat of the sun is the only thing which essentially affects the question. The quantity of heat falling from the sun during a given time upon a given portion of the earth's surface may be measured, and from this it can be calculated how much heat in a given time is sent out from the entire sun. Such measurements have been made by the French physicist Pouillet, and it has been found that the sun gives out a quantity of heat per hour equal to that which a

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layer of the densest coal 10 feet thick would give out by its combustion; and hence in a year a quantity equal to the combustion of a layer of 17 miles. If this heat were drawn uniformly from the entire mass of the sun, its temperature would only be diminished thereby 1½rd of a degree Centigrade per year, assuming its capacity for heat to be equal to that of water. These results can give us an idea of the magnitude of the emission, in relation to the surface and mass of the sun; but they cannot inform us whether the sun radiates heat as a glowing body, which since its formation has its heat accumulated within it, or whether a new generation of heat by chemical processes takes place at the sun's surface. At all events the law of the conservation of force teaches us that no process analogous to those known at the surface of the earth, can supply for eternity an inexhaustible amount of light and heat to the sun. But the same law also teaches that the store of force at present existing, as heat, or as what may become heat, is sufficient for an immeasurable time. With regard to the store of chemical force in the sun, we can form no conjecture, and the store of heat there existing can only be determined by very uncertain estimations. If, however, we adopt the very probable view, that the remarkably small density of so large a body is caused by its high temperature, and may become greater in time, it may be calculated that if the diameter of the sun were diminished only the ten-thousandth part of its present length, by this act a sufficient quantity of heat would be generated to cover the total emission for 2100 years. Such a small change besides it would be difficult to detect even by the finest astronomical observations.

Indeed, from the commencement of the period during which we possess historic accounts, that is, for a period of about 4000 years, the temperature of the earth has not sensibly diminished. From these old ages we have certainly no thermometric observations, but we have information regarding the distribution of certain cultivated plants, the vine, the olive tree, which are very sensitive to changes of the mean annual temperature, and we find that these plants at the present moment have the same limits of distribution that they had in the times of Abraham and Homer; from which we may infer backwards the constancy of the climate.

In opposition to this it has been urged, that here in Prussia the German knights in former times cultivated the vine, cellared their own wine and drank it, which is no longer possible. From this the conclusion has been drawn, that the heat of our climate has diminished since the time referred to. Against this, however, Dove has cited the reports of ancient chroniclers, according to which, in some peculiarly hot years, the Prussian grape possessed somewhat less than its usual quantity of acid. The fact

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also speaks not so much for the climate of the country as for the throats of the German drinkers.

But even though the force store of our planetary system is so immensely great, that by the incessant emission which has occurred during the period of human history it has not been sensibly diminished, even though the length of the time which must flow by, before a sensible change in the state of our planetary system occurs, is totally incapable of measurement, still the inexorable laws of mechanics indicate that this store of force, which can only suffer loss and not gain, must be finally exhausted. Shall we terrify ourselves by this thought? Men are in the habit of measuring the greatness and the wisdom of the universe by the duration and the profit which it promises to their own race; but the past history of the earth already shows what an insignificant moment the duration of the existence of our race upon it constitutes. A Nineveh vessel, a Roman sword awakes in us the conception of grey antiquity. What the museums of Europe show us of the remains of Egypt and Assyria we gaze upon with silent astonishment, and despair of being able to carry our thoughts back to a period so remote. Still must the human race have existed for ages, and multiplied itself before the pyramids or Nineveh could have been erected. We estimate the duration of human history at 6000 years; but immeasurable as this time may appear to us, what is it in comparison with the time during which the earth carried successive series of rank plants and mighty animals, and no men; during which in our neighbourhood the amber-tree bloomed, and dropped its costly gum on the earth and in the sea; when in Siberia, Europe and North America groves of tropical palms flourished; where gigantic lizards, and after them elephants, whose mighty remains we still find buried in the earth, found a home? Different geologists, proceeding from different premises, have sought to estimate the duration of the above creative period, and vary from a million to nine million years. And the time during which the earth generated organic beings is again small when we compare it with the ages during which the world was a ball of fused rocks. For the duration of its cooling from 2000° to 200° Centigrade, the experiments of Bishop upon basalt show that about 350 millions of years would be necessary. And with regard to the time during which the first nebulous mass condensed into our planetary system, our most daring conjectures must cease. The history of man, therefore, is but a short ripple in the ocean of time. For a much longer series of years than that during which man has already occupied this world, the existence of the present state of inorganic nature favourable to the duration of man seems to be secured, so that for ourselves and for long

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generations after us we have nothing to fear. But the same forces of air and water, and of the volcanic interior, which produced former geological revolutions, and buried one series of living forms after another, act still upon the earth's crust. They more probably will bring about the last day of the human race than those distant cosmical alterations of which we have spoken, and perhaps force us to make way for new and more complete living forms, as the lizards and the mammoth have given place to us and our fellow-creatures which now exist.

Thus the thread which was spun in darkness by those who sought a perpetual motion has conducted us to a universal law of nature, which radiates light into the distant nights of the beginning and of the end of the history of the universe. To our own race it permits a long but not an endless existence; it threatens it with a day of judgment, the dawn of which is still happily obscured. As each of us singly must endure the thought of his death, the race must endure the same. But above the forms of life gone by, the human race has higher moral problems before it, the bearer of which it is, and in the completion of which it fulfils its destiny.

Note to Page 506.

I must here explain the calculation of the heat which must be produced by the assumed condensation of the bodies of our system from scattered nebulous matter. The other calculations, the results of which I have mentioned, are to be found partly in J. R. Mayer's papers, partly in Joule's communications, and partly by aid of the known facts and method of science: they are easily performed.

The measure of the work performed by the condensation of the mass from a state of infinitely small density, is the potential of the condensed mass upon itself. For a sphere of uniform density of the mass M, and the radius R, the potential upon itself V—if we call the mass of the earth m, its radius r, and the intensity of gravity at its surface g,—has the value

$$V=\frac{3}{5}\cdot\frac{r^2M^2}{Rm}\cdot g.$$

Let us regard the bodies of our system as such spheres, then the total work of condensation is equal to the sum of all their potentials on themselves. As, however, these potentials for different spheres are to each other as the quantity $\frac{M^2}{R}$, they all vanish in comparison with the sun; even that of the greatest planet, Jupiter, is only about the one hundred-thousandth part of that of the sun; in the calculation, therefore, it is only necessary to introduce the latter.

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To elevate the temperature of a mass M of the specific heat σ, t degrees, we need a quantity of heat equal to $M\sigma t$; this corresponds, when Ag represents the mechanical equivalent of the unit of heat, to the work $AgM\sigma t$. To find the elevation of temperature produced by the condensation of the mass of the sun, let us set

$$AgM\sigma t = V;$$

we have then

$$t = \frac{3}{5} \cdot \frac{r^2 M}{A \,.\, R \,.\, m \,.\, \sigma}.$$

For a mass of water equal to the sun we have $\sigma = 1$; then the calculation with the known values of A, M, R, m, and r, gives

$$t = 28611000^\circ \text{ Cent.}$$

The mass of the sun is 738 times greater than that of all the planets taken together; if, therefore, we desire to make the water mass equal to that of the entire system, we must multiply the value of t by the fraction $\frac{738}{739}$, which makes hardly a sensible alteration in the result.

When a spherical mass of the radius R condenses more and more to the radius R_1, the elevation of temperature thereby produced is

$$\vartheta = \frac{3}{5} \cdot \frac{r^2 M}{A \,.\, m\sigma} \left\{ \frac{1}{R_1} - \frac{1}{R_0} \right\},$$

or

$$= \frac{3}{5} \cdot \frac{r^2 M}{A R_1 m\sigma} \left\{ 1 - \frac{R_1}{R_0} \right\}.$$

Supposing, then, the mass of the planetary system to be at the commencement, not a sphere of infinite radius, but limited, say of the radius of the path of Neptune, which is six thousand times greater than the radius of the sun, the magnitude $\frac{R_1}{R_0}$ will then be equal to $\frac{1}{6000}$, and the above value of t would have to be diminished by this inconsiderable amount.

From the same formula, we can deduce that a diminution of $\frac{1}{10000}$ of the radius of the sun would generate work in a water mass equal to the sun, equivalent to 2861 degrees Centigrade. And as, according to Pouillet, a quantity of heat corresponding to $1\frac{1}{4}$ degree is lost annually in such a mass, the condensation referred to would cover the loss for 2289 years.

If the sun, as seems probable, be not everywhere of the same density, but is denser at the centre than near the surface, the potential of its mass and the corresponding quantity of heat will be still greater.

Of the now remaining mechanical forces, the *vis viva* of the rotation of the heavenly bodies round their own axes is, in comparison

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with the other quantities, very small, and may be neglected. The *vis viva* of the motion of revolution round the sun, if μ be the mass of a planet, and ρ its distance from the sun, is

$$L = \frac{gr^2 M\mu}{m}\left\{\frac{1}{R} - \frac{1}{2\rho}\right\}.$$

Omitting the quantity $\frac{1}{2\rho}$ as very small compared with $\frac{1}{R}$, and dividing by the above value of V, we obtain

$$\frac{L}{V} = \frac{5}{3}\frac{\mu}{M}.$$

The mass of all the planets together is $\frac{1}{738}$ of the mass of the sun, hence the value of L for the entire system is

$$L = \frac{1}{453}.V.$$

LXV. *Remarks on the relative value of the Ozonometers of* Drs. Schönbein *and* Moffat, *based upon daily observations for eighteen months at Bedford. By* T. Herbert Barker, *M.D.**

THE discovery, in 1840, by Dr. Schönbein, of that principle in the atmosphere to which he gave the name *ozone*, has excited considerable interest throughout Europe. The indications already noticed of a probable connexion between this newly-observed agent and certain states of the atmosphere affecting the public health, especially the remarkable disappearance of all signs of its existence during the epidemic prevailing in the summer of 1854, together with several less striking, yet important coincidences already recorded by ozone observers, might tempt the speculative mind to indulge in premature theorizing. But it must be remembered, that, while we have already ample materials for speculation, we have, comparatively speaking, but a scanty supply of those well-ascertained facts of coincidence on which a sound theory must be based. To gain a true insight into the nature and the effects of ozone, we must first *add greatly to the number of our observations.*

Taking this view of the stage at which we have already arrived in the investigation of ozone, it becomes highly important and interesting to inquire by what means its presence in the atmosphere may be most surely and readily detected. It is obvious,

* Communicated by the Author.

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that before we can safely say anything respecting its probable *effects*, we must first be sure of its presence or its absence. Nothing could be more mortifying to a medical observer than to find, after he had explained certain phænomena as connected with the *absence* of ozone, that this principle was in fact *present* at those times when the said phænomena occurred. To prevent such an error, we must have the best possible test-papers or ozonometers.

For the benefit of readers who may require such information, we may briefly explain here, that the ozonometer consists simply of strips of paper prepared with iodide of potassium and starch. These papers are suspended so as to be exposed to the free access of air, but not to the direct rays of the sun. The paper, when affected by ozone, is found tinged with various shades of brown, of which the intensity is measured by a scale of ten gradations. Dr. Schönbein recommends that test-paper, prepared according to his own formula, should be suspended in a spot to which the air, but not the direct rays of light, may have free access, and that it should be removed from the neighbourhood of stables, manure-heaps, &c., where the gases generated might vitiate the observations. The brown tinge of the ozonometer is produced by the decomposition of the iodide of potassium—the oxygen of the ozone (which Faraday defines as oxygen in an allotropic condition) combining with the potassium, and setting free the iodine, which now forms iodide of starch. In its dry state this new combination is of a brown colour; but when moistened with water it assumes a blue tint, of which the intensity indicates the quantity of ozone. This test-paper becomes colourless shortly after its immersion in water.

The next ozonometer of which we have to speak is the paper prepared by the formula of Dr. Moffat of Hawarden, the first of ozone observers in our own country. His recorded observations commenced in 1848. In his directions for the use of his own ozonometer, Dr. Moffat requires that the test-paper be suspended in a box, so perforated as to admit a free passage of air, but not of light. When thus exposed to the action of air containing ozone, the prepared paper acquires a brown tinge, varying in intensity from 0° to 10°. Dr. Moffat's plan does not require the moistening of the paper to procure the blue tint. Provided it be kept in the dark, his test-paper will retain its brown tints for a long period, even for two or three years.

We have now to present to our readers the results of a series of careful comparative observations on these two test-papers for ozone. The observations made at Bedford commenced, in the first instance, in October 1853, and when it had become appa-

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rent that the test-papers of Schönbein and Moffat varied considerably in susceptibility of colouring, a formal course of comparative observations was commenced in November 1854, and has been continued to the present time.

Other observers have instituted comparative trials of the two ozone papers; but the extent, as well as the numerical precision, of the results obtained at Bedford must render them valuable to all who wish to employ the utmost precaution in observations on ozone.

In order that the two papers might be submitted to precisely the same test, a box was constructed on the plan recommended by Mr. Prince of Uckfield*. It freely admitted the air, without a ray of light. Schönbein's papers were procured from Mr. John Cox of Rye Lane, Peckham (who was, for some time, the accredited agent for both the test-papers), the others were received from Dr. Moffat himself. The two kinds of test-paper were suspended, side by side, in the centre of the box, and, to avoid any accidental failure of a slip, two of each kind were used. Observations were recorded daily at 9 A.M., not oftener, except at certain times, when ozone was present in more than its average quantity, and the papers were found highly discoloured at 3 P.M., the ordinary hour for meteorological observations. The papers were changed only when they had become tinged. At various times they were left during intervals varying from one to seventeen days without suffering any change of colour.

In most instances the discoloured papers were compared with Dr. Moffat's scale of brown tints; but sometimes, to vary the mode of testing, they were immersed in water according to Schönbein's instructions. In each process the results were relatively the same. Thus, if a coloured paper agreed with No. 5 of Dr. Moffat's brown tints, before immersion, it also agreed with No. 5 of Schönbein's scale of purple tints, after immersion.

With these explanations of the carefulness and fairness of the observations made at Bedford, we now lay before our readers the following table of the results obtained:—

* Its construction is described and figured in a paper given in the 'Association Medical Journal,' March 3, 1854.

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Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1854.		
Dec. 4.	1	2
10.	0	1
19.	1	2
28.	1	2
31.	1	2
	4	9
1855.		
Jan. 1.	0	1
8.	0	1
17.	2	3
18.	0	2
20.	2	2
21.	3	5
30.	1	3
31.	2	3
	10	20
Feb. 1.	4	6
2.	1	1
3.	2	3
7.	4	4
8.	2	4
9.	6	7
10.	1	2
12.	0	2
16.	1	1
20.	1	3
22.	0	2
25.	0	1
	22	36
Mar. 3.	5	6
8.	2	4
13.	2	4
15.	0	2
18.	2	4
19.	0	3
21.	1	4
22.	2	5
23.	4	8
24.	0	2
25.	1	2
29.	0	2
	19	46

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1855.		
April 2.	1	1
9.	2	4
10.	2	3
11.	1	2
12.	3	8
14.	0	2
18.	1	2
21.	2	4
22.	0	2
25.	1	3
26.	0	2
29.	2	4
30.	4	6
	19	43
May 1.	1	3
2.	3	6
4.	6	8
5.	1	2
6.	0	1
7.	0	4
8.	6	7
9.	0	3
10.	0	2
11.	0	2
12.	0	3
13.	0	1
14.	7	10
15.	0	2
17.	0	4
19.	0	2
21.	0	4
23.	0	3
25.	0	1
26.	0	1
27.	0	2
28.	0	3
29.	2	5
30.	2	4
31.	6	10
	34	93
June 1.	0	3
4.	0	3
5.	1	3
6.	2	5
7.	0	3
11.	2	4
12.	2	4

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1855.		
Jun. 14.	4	6
15.	3	6
18.	1	3
19.	0	2
22.	2	6
25.	2	6
26.	5	8
29.	2	6
	26	68
July 3.	2	4
6.	4	6
8.	2	4
10.	3	6
11.	0	2
12.	2	4
17.	1	3
21.	0	2
23.	2	4
27.	3	5
31.	0	2
	19	42
Aug. 2.	2	4
7.	4	7
9.	2	4
12.	2	4
13.	3	4
15.	0	2
18.	6	8
19.	1	2
24.	0	2
29.	0	2
31.	2	4
	22	43
Sept. 4.	2	4
7.	4	6
11.	1	3
14.	2	3
18.	2	4
19.	1	2
23.	4	6
27.	4	6
28.	0	4
30.	1	3
	21	41

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1855.		
Oct. 3.	4	6
7.	2	3
11.	2	4
14.	3	4
16.	2	3
18.	3	4
21.	0	1
25.	0	4
26.	5	9
29.	0	5
30.	1	5
31.	9	8
	31	56
Nov. 1.	9	10
3.	8	9
4.	5	7
6.	0	3
7.	0	3
8.	0	2
25.	2	4
	24	38
Dec. 4.	0	3
6.	5	5
7.	0	4
8.	0	2
19.	7	9
20.	6	8
21.	2	4
27.	6	7
29.	0	2
30.	2	3
	28	47
1856.		
Jan. 10.	8	10
11.	6	9
12.	1	3
22.	10	10
24.	3	5
25.	2	4
	30	41

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Table (*continued*).

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1856.		
Feb. 6.	3	7
7.	0	1
14.	4	4
18.	1	4
19.	2	3
	10	19
Mar. 1.	1	3
2.	0	2
3.	3	6
4.	0	7
7.	0	3
13.	0	4
14.	1	4
24.	0	1
25.	0	1
26.	0	2
27.	0	1
29.	0	2
30.	0	1
	5	37

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1856.		
April 1.	0	2
4.	0	4
6.	0	4
8.	0	2
9.	0	3
13.	0	3
15.	6	6
16.	2	4
17.	1	4
18.	0	5
23.	0	2
26.	0	3
27.	2	5
28.	2	3
	13	50
May 2.	0	3
3.	0	2
4.	0	3
5.	0	3
7.	0	3
9.	0	5

Date.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1856.		
May 10.	3	4
11.	0	3
12.	0	1
18.	4	5
19.	4	6
22.	0	2
23.	0	3
24.	0	3
26.	0	4
28.	0	3
30.	0	5
31.	3	6
	14	64

TOTAL. Month.	Schönbein's Ozonometer.	Moffat's Ozonometer.
1854.		
Dec. ...	4	9
1855.		
January	10	20
Febuary	22	36
March	19	46
April ...	19	43
May ...	34	93
June ...	26	68
July ...	19	42
August	22	43
Sept. ...	21	41
October	31	56
Nov. ...	24	38
Dec. ...	28	47
1856.		
January	30	41
Febuary	10	19
March	5	37
April ...	13	50
May ...	14	64
	351	793

A few brief remarks may serve to indicate the importance of the results thus obtained.

During the eighteen months over which the observations extended, there were 122 days in which Schönbein's paper indicated the presence of ozone; while there were 207 days in which Moffat's papers were discoloured. In other words, in 85 days, out of 207, Schönbein's papers failed to record the action of ozone! On the other hand, Moffat's papers *never* failed to receive a tinge when Schönbein's indicated ozone. It is true, on one occasion (October 31st, 1855) Schönbein's papers were more deeply tinged than Moffat's; but this may safely be passed over as a mere accident, when weighed against the contrary results of 197 observations.

The total amount of ozone indicated by Schönbein's paper, during the entire period of observation, is represented by the number 351, while Moffat's paper registered 793.

The mean monthly amount by Schönbein's was 19·50; by Moffat's 44·05.

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The mean daily amount by Schönbein's was 1·70; by Moffat's 3·83.

Such results are too plain to require any lengthened comments on the relative value of the two ozonometers.

There can no longer remain a doubt that the papers prepared by Moffat's formula are more sensitive than those of Schönbein in the ratio of 2·3 to 1·0. The advantages possessed by the former have been proved by observations in other localities besides Bedford. Mr. Glaisher, in his valuable report on the "Meteorology of London and its relation to the Epidemic of Cholera," has stated that "the papers prepared by Dr. Moffat were more sensitive than those of Dr. Schönbein, and accordingly indicated the presence of ozone when none was indicated by those of Schönbein" (page 71). It appears clear, therefore, that our first step to be taken in further observations on ozone is to adopt without delay, and uniformly, Dr. Moffat's formula for test-paper*.

The next step must be, not to theorize on scanty data of coincidences, but to extend, as widely as possible, the field of observation, and carefully to record results. Our progress may seem slow, but it is by such patient labours that the secrets of nature's laboratory must be disclosed.

In conclusion, we would express a hope that meteorologists who have not yet added the ozonometer to their observatories will no longer delay to do so. It must be by the cooperation of many that certain and great results will be attained. Each may feel that he is doing little—that he is only collecting, from day to day, a few grains of evidence.

> ..... "Trahit quodcunque potest, atque addit acervo
> Quem struit, ..."

But slight exertions become important when regarded as contributions to a great result. Thus we must go on from day to day, from year to year; and, by and bye, the properties of ozone will be wrested, like other secrets, from nature. We shall then be able to contrast the certainties known with the hints and suspicions of our present stage of inquiry, even as now we may contrast our certain knowledge of heat, of electricity, of magnetism, of light, and of many chemical phænomena, with the mere forebodings of a period not yet very remote.

Bedford, June 2nd, 1856.

* As considerable care is required in the thorough saturation of the paper with solution of iodide of potassium and starch, we would suggest to observers desirous of obtaining trustworthy results, that it would be most advisable to obtain the prepared paper from the accredited agents, Negretti and Zambra, Hatton Garden, London. This may be done at a very reasonable expense.

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LXVI. *On the Law of Electric Discharge.* *By* Dr. P. Riess.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the May Number of your Magazine there is a paper of twenty pages, entitled "On a General Law of Electric Discharge, by Sir W. Snow Harris," which would necessitate many observations if all the errors contained in it were to be corrected. As, however, this paper possesses rather a personal than a scientific interest, I will confine myself to a very few remarks, which may easily be added to by those familiar with the subject.

The appearance of the above paper is not to be laid to my charge. M. De la Rive, in his most recent English work, has given in part to my investigations upon electrical heat that consideration which they have long enjoyed in German works, and has not made use of Harris's labours upon the same subject. That Sir W. Harris should gladly give importance to his own researches is of course very natural; but whether he has done it in a proper manner others may judge. For my own part I have only to observe, that it does not appear to me to be altogether just that he should make me answerable for statements which are not derived from my original works, that he should ascribe to me "a systematic disparagement" of his scientific labours, and lastly, that he should allow himself (p. 359 at the bottom) to express a suspicion regarding me. Against the two first proceedings I hereby remonstrate, and his suspicion I beg to say is totally unwarranted.

The subject which, according to the title, forms the principal contents of the paper is easily dispatched, perhaps to the satisfaction of the author. He has long since set up a law upon the dependence of the electrical heat upon the charge of the battery. I ascertained this law to be incorrect, and set another in its place. Sir William Harris now seeks to bring the two laws into agreement by the inadmissible process of giving a signification to the symbol s in my formula $T=\frac{Q^2}{s}$ different from that given to it by me. He understands by it the resistance in the circuit, although in the formula (which constitutes only a portion of the general heat-formula) this ought not at all to be introduced, as it is supposed to be constant. If the resistance in the circuit is variable, not only this, but also the resistance in the battery is expressed, which must have entirely escaped Sir W. Harris, as he asserts (p. 354) that I had overlooked the latter resistance. The question for him therefore is a simple one, whether the formula referred to with the signification which I attached to the symbol s be correct or false, and he may endea-

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vour to ascertain the latter by experiment. But most certainly experiments must be justified by theory, and exactly performed and described if any weight is to be attached to them.

Far more irksome to me than this explanation, is the discussion of the other portion of the memoir which consists of the frequently-repeated but unjustified assertion that my own investigations on heat, by which, at the expense of much time and trouble, I have arrived at simple laws, are nothing more than mere reproductions of the author's previous experiments. It is quite true, and has been acknowledged by me, that his unsatisfactory and inconclusive experiments were the cause of my investigations, which were rewarded with a better result. But the credit of having opened up an investigation, although it is by no means small, rarely satisfies those who led the way; they require more, even though they should only acquire it in the eyes of those who are unacquainted with the subject. "It is a course by no means uncommon in the history of physical science."

Sir William Harris had made a great many electrical experiments, and from them, without possessing the necessary knowledge, had deduced numerous results which always remained unintelligible to me, as they were often in contradiction to well-known facts, and even with the experiments of the author himself. I was not aware that any one, either here or in England, had found these results more intelligible than I had done. Of course, now that my endeavours of many years have made known the laws of electrical heat, some experiments out of this mass have become more comprehensible, but it is certain that even these experiments, cancelled by other contradictory ones, would never have rendered the discovery of these laws possible. Nothing remained for me to do, in the statement of my investigations, except to bring the existence of these experiments to the knowledge of the reader, and then, on my own account, to place the subject, which had been confused in the highest degree, in a clear light. My first memoir on electric heat (1837) commences with the mention of Harris's most recent work, a memoir which the author often quotes, and in which he had summed up his previous observations *. As besides electrical heat other phænomena are investigated in this memoir, I gave a complete abstract of it in Dove's *Repertorium der Physik*, Berlin, 1838, and described the instruments and experiments mentioned therein as exactly as the statements of the author enabled me to do. All the results of this memoir which I considered as correct I have since made use of under the name of the author with suitable acknowledgments.

* "On some Elementary Laws of Electricity," Phil. Trans. 1834.

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In my investigations of heat, it was natural that I should submit the instruments and processes employed by the author to a rigid criticism, and reject them when they did not stand the test. It is not true that I ascribed the unsuccessful experiments of Sir W. Harris to the faulty arrangement of his thermometer. My words, that the experiments were not made with the necessary care (Pogg. *Annalen*, vol. xl. p. 885), cannot be referred to the thermometer, as immediately afterwards, in the description of my instrument, I say that the bulb (consequently the most essential portion) was arranged according to Harris's description. A greater simplicity and sensibility of the thermometer certainly appeared to me to be desirable.

My objections, if my memory is correct after so long a time, referred to the totally inadmissible process of the author of effecting the discharge of the battery by means of a ball which was obliged to shatter a glass disc lying upon the battery. I was compelled at once to reject the "unit-jar" of Sir W. Harris, as I perceived that during the charging the unit of the amount of electricity would continually decrease, and that the more rapidly the smaller the charged battery is. I adopted another method of measuring the amount of electricity, because it was justified by theory and proved by practice, and in employing it I did not forget to state that it was described by Haldane in 1800. I have hitherto supposed that I was the first to make a practical use of this process, but I am now informed by Sir W. Harris that it had already been employed by him in 1830. My error is very excusable, for it was difficult to believe that a physicist should have been acquainted with a correct method of measurement, and have rejected it in favour of an incorrect one. I have sufficiently acknowledged the merits of Sir W. Harris in the improvement of the electrical thermometer, by frequently describing and once figuring it in the form which he represents as the most perfect. But I must affirm that he is still unacquainted with the use of the thermometer in the demonstration of the laws of electrical heat, as he is still unaware of the necessity of employing a calculation to render the data capable of comparison when the wires are changed in the thermometer. Nor can I at all agree with Sir W. Harris in his favourite idea, that his thermometer is a peculiar instrument essentially different from Kinnersley's. The disadvantageous vertical position of the tube with the fluid is common to both instruments. The air-holder is globular in the one and cylindrical in the other; in the one the wire is extended by the fastening of its two ends, in the other it is fastened to the lid of the air-holder, stretched by a weight and lowered into the air-holder. These are differences of construction which may perhaps affect the convenience

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and exactitude of the experiments, but cannot constitute an essential difference in the instruments. My claims to the right of invention appear to be much stronger than those of Sir W. Harris, for I have introduced changes into the electroscope of Behrens and Fechner of a much more important nature than those made by him in Kinnersley's thermometer, but never entertained a thought of describing the improved electroscope as a new instrument.

Lastly, I have to remark, that the author's assertion that Kinnersley only employed his thermometer for the illustration of the mechanical force of the electrical explosion in the air is completely destitute of foundation. In 1761 Kinnersley also made use of his thermometer to test the amount of heat which the discharge of a Leyden jar "produced in a strip of wet writing-paper, a wet flaxen and woollen thread, a blade of green grass, a filament of green wood, a fine silver thread, a very small brass wire and a strip of gilt paper." It is inconceivable that Sir W. Harris did not know this, not because it stands in one of my memoirs to which he has referred, but because these experiments are mentioned in a classical English work, Franklin's immortal 'Experiments and Observations,' which is certainly the best known and most widely diffused of all the works that have ever been written on electricity.

I have the honour to be, Gentlemen,
Your obedient Servant,

Berlin, May 19, 1856. P. Riess.

LXVII. *Note on the Production of Formic Æther.* By Arthur H. Church, *F.C.S.**

OXALOVINIC acid, $C^8H^6O^8$, experiences under certain conditions a metamorphosis of considerable interest. Just as oxalic acid, when heated with pumice-stone, with sand, or, as recently pointed out by Berthelot, with glycerine, yields formic and carbonic acids, as expressed by the following equation,—

$$C^4H^2O^8 = C^2H^2O^4 + 2CO^2,$$

so oxalovinic acid, when similarly treated, yields formiate of æthyle and carbonic acid, thus:—

$$C^4\begin{matrix}C^4H^5\\H\end{matrix}O^8 = C^2\begin{matrix}C^4H^5\\H\end{matrix}O^4 + 2CO^2.$$

Oxalovinic acid, even in the impure state in which it occurs when prepared from its potash salt by the addition of an equivalent quantity of sulphuric acid, furnishes, when heated with gly-

* Communicated by the Author.

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cerine at 100° C., so large a quantity of formic æther as to be an excellent source of that substance. The greater part of the oxalovinic acid which I employed in my experiments was prepared by the action of pure hydrate of potassa upon oxalic æther, using equivalents of the two bodies; yet on testing a mixture of absolute alcohol and dry oxalic acid left in contact since June 1854, the presence of a considerable amount of the acid oxalate of æthyle was indicated; and the fluid part of this mixture, treated with an equal volume of glycerine, yielded on distillation about one-fourth its weight of formic æther.

The formiate of æthyle produced in the manner above described agreed in odour, boiling-point, and specific gravity, with the formic æther obtained by the ordinary methods; but in order to satisfy myself of its identity, I burnt a portion of the body with oxide of copper in the usual manner; the following are the details of the analysis:—

0·2015 grm. of substance gave 0·36 grm. carbonic acid, and 0·14525 grm. water.

The theoretical and experimental per-centages of carbon and hydrogen in formic æther are as follows:—

	Theory.	Experiment.
Carbon . . .	48·68	48·7
Hydrogen . .	8·1	8·0

From the results of some qualitative experiments, I think I am justified in concluding that formiate of methyle may be produced from oxalo-methylic acid by the action of glycerine; and that it would not be without interest to examine the behaviour with glycerine of the other members of that series of bibasic acids to which oxalic acid belongs, not indeed only the acids, but the acid æthyle and methyle compounds.

LXVIII. *On the Constitution of Green and Blue Ultramarine.* By E. Breunlin *of Weissenau**.

ONE of the most beautiful and important mineral colours is that called ultramarine. Although we have long known it, and it is now produced in large quantities and is extensively applied, its true constitution has never been clearly ascertained. The theories hitherto proposed have been grounded on the mode of its preparation, and on a few analyses, but none of them has obtained authority; they are not precise, and prove little by means of numbers; perhaps also the materials used for the analysis were faulty.

* Translated by Dr. E. Atkinson from the *Annalen der Chemie und Pharmacie* for March 1856.

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From the increasing production of ultramarine, and its importance in trade and industry, it appeared interesting to explain by chemical analysis the nature of the blue compound; and by the kindness of one of the most celebrated ultramarine makers in Germany, I obtained very beautiful samples of ultramarine, blue as well as green.

Porcelain clay, or a similarly constituted artificial silicate, is mixed with soda and sulphur, and ignited without access of air until the mass has caked together; it is then ground and washed. The resulting powder is either again ignited with soda and sulphur, or gently heated with access of air; the blue colour then appears.

The different sorts of blue ultramarine exhibit different physical properties, while their chemical relations are the same. The colour varies from a delicate cærulean to a fiery dark blue with a tinge of red. The lighter sorts form a compact, dense powder; the darker kinds are looser and velvety. Green ultramarine has no fiery colour; its shades vary from apple-green to blue-green.

Ultramarine is not moistened by water, but it is by alcohol, even greatly diluted. When treated with water, some sulphate of lime is dissolved; but neither a sulphite, hyposulphite, nor a sulphide can be detected in the solution.

Sulphuretted hydrogen is evolved when ultramarine is treated with acids; even with dilute acetic acid the colour disappears more or less rapidly. The most beautiful shades are the soonest decomposed. Many ultramarines resist the action of acids more energetically than others; the green are the easiest decomposed.

If strong hydrochloric acid in excess be poured on ultramarine, an odour is evolved which irritates the eyes, similar to that observed in the preparation of polysulphuretted hydrogen when polysulphide of calcium is treated with excess of strong acid.

When ultramarine is decomposed by acid, a whitish gelatinous fluid is obtained, which does not filter clearly. The cloudiness arises from finely divided sulphur; it is more decided in the blue than in the green ultramarine, and shows the presence of a higher sulphide which is present in ultramarine, and contributes essentially to the colour. Sulphur, clay, and silica remain on the filter; the filtrate contains the chlorides of aluminium, iron, and sodium, and sulphate of lime.

When strongly ignited in air, both ultramarines lose their colour, which becomes first dull and dirty, and then quite disappears. If green ultramarine be heated with pentasulphide of sodium, and the mass washed out and gently heated in the air, it becomes blue. The reason of this change will be found in the different constitution of blue and green ultramarine.

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I made analyses of five kinds of blue ultramarine (I. to V.), and of two kinds of green (VI. VII.), with the following results:—

	I.	II.	III.	IV.	V.	VI.	VII.
Silica . . .	37·4	40·9	35·5	36·3	36·6	38·4	38·8
Alumina . .	30·0	24·2	28·4	25·9	25·0	27·4	28·3
Oxide of iron .	1·3	0·5	0·6	3·1	0·9	0·6	0·9
Soda . . .	14·9	16·3	19·2	21·0	17·2	16·9	13·9
Sodium . . .	2·8	3·2	1·9	2·1	3·2	5·3	5·5
Sulphur *a* .	2·0	2·2	1·3	1·4	2·2	3·7	3·8
Sulphur *b* .	7·1	8·5	4·9	5·8	8·7	3·5	5·7
Lime . . .	0·5	0·8	0·6	1·1	1·0	0·8	0·9
Sulphuric acid	2·3	1·3	3·1	2·7	2·0	0·5	0·6
Clay . . .	2·8	1·5	2·0	2·3	2·8	1·7	1·0
	101·1	99·4	100·5	101·7	99·6	98·7	99·4

In these analyses, sulphur *a* is the quantity of sulphur liberated as sulphuretted hydrogen when ultramarine is treated with acid, and sulphur *b* the quantity of sulphur which falls as milk of sulphur.

The analyses were executed in the following manner:—

A quantity of ultramarine, dried at 100° C., was drenched in a beaker with hydrochloric acid, and completely decomposed. It was filtered; silica, sulphur, and undecomposed clay remained on the filter, and were washed out, and the silica separated from the clay by solution in caustic potash, from which it was separated by acid, and determined. The filtrate from the clay, sulphur and silica was evaporated to dryness, the residue heated above 100° C., then moistened with HCl, and digested with water. The silica which had been dissolved in hydrochloric acid remained behind, was filtered, and estimated along with the other.

In the filtrate, alumina and oxide of iron were precipitated by ammonia, and separated from each other by potash; the lime was precipitated by oxalate of ammonia, and the soda determined as sulphate of soda.

Another portion of ultramarine was fused with soda and nitre in a platinum crucible; the mass swelled up from the carbonic acid liberated, and became bluish-green, green, and then colourless.

From the solution in acidulated water, silica was separated in the usual manner, and the sulphuric acid determined as sulphate of baryta. This analysis gave the whole of the sulphur, along with the sulphuric acid contained already as such.

Another portion was treated with hydrochloric acid, the silica separated, and in the filtrate sulphuric acid precipitated by

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chloride of barium. The sulphuric acid here obtained is contained in undecomposed ultramarine, in combination with lime as gypsum, which is a never-failing admixture in clay. The quantity of sulphur liberated as sulphuretted hydrogen in the decomposition of ultramarine by acid was thus estimated.

In a flask of a litre capacity, about 0·5 grm. ultramarine was brought, and the flask filled with water. About 10 drops of thin starch-paste were added, and about 10 to 15 cub. centims. of strong hydrochloric acid. The blue colour disappeared gradually, and became finally milky white. By means of a burette, a solution of 5 grms. iodine in 1000 cub. centims. water was dropped in, the mixture being continuously shaken until the characteristic colour of the iodine appeared. The sulphuretted hydrogen could be estimated to 0·1 cub. centim., for its quantity in the liquid never reached that point at which its determination becomes incorrect.

As an example of the mode of calculating the sulphuretted hydrogen, the determination No V. may be given. In this 17·6 cub. centims. of iodine were employed, which contained 0·088 grm., corresponding to 0·0117 grm., or 2·356 per cent. sulphuretted hydrogen, or 2·217 per cent. sulphur; for 1 equiv. sulphuretted hydrogen requires 1 equiv. iodine to decompose it into hydriodic acid and sulphur.

From the total quantity of sulphuric acid found, that portion was subtracted which was contained as such in the ultramarine, and the remainder was calculated as sulphur. From this the quantity of sulphur was subtracted which was evolved as sulphuretted hydrogen, and the remainder gave the quantity of sulphur separated as milk of sulphur in the decomposition of ultramarine.

The theories of the constitution of ultramarine have been as various as the researches published on them. One point has been agreed on, that with the exception of iron no heavy metal is contained in ultramarine; but whether the product was a compound of silica, alumina, soda, and sulphur, or whether the iron united with sulphur was the colouring principle, has been undecided.

Elsner thinks that a small quantity of iron is essential to the blue colour, but an excess injurious; and Kressler and Prückner hold a similar opinion. Guyton Morveau held that the colouring principle was sulphide of iron. This opinion has been extensively received.

Brünner, on the other hand, says that the iron plays no important part, for a mixture of materials free from iron gave a blue, the same as that from materials containing iron.

I have found that the iron, when present in considerable

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quantity, has an injurious action on the colour. It was long known that silica, soda, and alumina gave, under no circumstances, a blue colour; but from the investigations of the chemists above mentioned, as well as from the fact that ultramarine treated with strong acids lost its colour under evolution of sulphuretted hydrogen, it was evident that sulphur was the most essential constituent.

This has long been partially recognized, and it was assumed that the sulphur was in combination with iron.

Brünner considers that the sulphur is united with the soda, partly as monosulphide of sodium, and partly as sulphate of soda. His view is grounded on this experiment.

He took an ultramarine basis, which was of a greenish-blue colour, and ignited it several times with carbonate of soda and sulphur, by which the mass became darker; he then mixed it with finely divided sulphur, and heated it in the air. The mass increased in weight. The unburnt mass contained 5·198 per cent. sulphur. By burning, it increased in weight 10·16 per cent., and contained after the burning 12·81 per cent. sulphur. The increase of weight consisted of 7·618 per cent. sulphur + 2·542 per cent. oxygen = 10·16 per cent.

The analysis of the unburned ultramarine gave—

Silica	35·841
Alumina . . .	27·821
Lime	2·619
Oxide of iron .	2·475
Sodium . . .	18·629
Sulphur . . .	5·293
Oxygen . . .	7·422
	100·000

But since 100 parts in burning became 110·16, in which 12·81 sulphur are contained, the rest of the constituents remaining the same, the ultramarine after burning must consist of—

Silica	32·544
Alumina . . .	25·255
Lime	2·377
Oxide of iron .	2·246
Sodium . . .	16·910
Sulphur . . .	11·629
Oxygen . . .	9·039
	100·000

Brünner calculated and found that 20·157 parts of sulphate of soda corresponded to 9·039 per cent. oxygen: he subtracted the quantities of sodium and sulphur contained therein from 11·639

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sulphur and 16·91 sodium, which gave 7·084 sodium and 10·337 sulphur, or 17·421 monosulphide of sodium. This arrangement of the sulphur, sodium, and oxygen has, according to Brünner's results, some probability, but the 11·306 per cent. sulphuric acid can scarcely have been found. Besides, Brünner does not seem to have noticed, that in the decomposition of ultramarine by hydrochloric acid sulphur is eliminated, which proves the presence either of a polysulphide or of a hyposulphite of a metal; while on the supposition of monosulphide of sodium no sulphur should precipitate, but should all be evolved as sulphuretted hydrogen.

Elsner and Rammelsberg found that the quantity of sulphur was far more than sufficient to combine with the iron, and that it must be present as sulphide of sodium.

From the comportment of ultramarine with strong acids, Gmelin considers that the sulphur is partially present as hyposulphurous acid; for this acid decomposes with a stronger acid into sulphur and sulphurous acid, and the sulphurous acid coming in contact with sulphuretted hydrogen evolved at the same time, is decomposed into sulphuric acid, sulphur, and water.

It might also be assumed that ultramarine contained a sulphide along with sulphate, but it is not then conceivable how a sulphate as such should contribute to the colour, nor why the blue compound should not also be formed under exclusion of the air.

I have considered it right to adduce these theories of the colouring principle of ultramarine, and I will now explain my own, which I have formed from the results of my analyses.

If we glance at the constituents of ultramarine, we see that the chief quantities are silica, alumina, soda, and sulphur; oxide of iron, clay, sulphuric acid, and lime, are present in far smaller quantities. I hold them to be impurities, and in establishing a formula do not consider them.

The relation of ultramarine to oxidizing and deoxidizing agents, the products formed from its decomposition by hydrochloric acid, is a qualitative, and the results of the various analyses are quantitative indications, that in ultramarine, as Elsner has proved, the colouring principle is a metallic sulphide united with a silicate. The metallic sulphide in the blue is a higher one than in the green ultramarine.

I found that blue ultramarine contained pentasulphide and green bisulphide of sodium, and that these were united with a silicate of constant composition, which has the greatest similarity to that of a natural silicate, nepheline. It consists of

$$(2NaO)\,SiO^3 + 2(Al^2O^3\,SiO^3).$$

The colouring compound in blue ultramarine contains 2 equi-

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valents of silicate to 1 equivalent of pentasulphide of sodium, and that in green 1 equivalent of silicate to 1 equivalent of bisulphide of sodium. I will endeavour in what follows to develope the probability of this constitution.

Metallic Sulphide.—In the ultramarine analysed, I found for the quantity of sulphur liberated as sulphuretted hydrogen in the decomposition by hydrochloric acid S*a*, for the quantity precipitated as milk of sulphur S*b*, the following numbers:—

		I.	II.	III.	IV.	V.
Blue ultramarine	S*a*	1·9	2·2	1·3	1·4	2·2
	S*b*	7·1	8·4	4·8	5·8	8·6

which is almost in the proportion of 1 to 4.

		VI.	VII.
Green ultramarine	S*a* .	3·6	3·9
	S*b* .	3·4	5·7

No. VI. was pure green. In it S*a* : S*b* as 1 : 1. No. VII. was bluish-green.

The sulphur is united with sodium. The quantity of sodium corresponding to S*a* was calculated (the weight of soda corresponding to this quantity of sodium was calculated and subtracted from the amount of soda found by the analysis).

Na S*a* is the monosulphide of sodium. Na + S*a* + S*b* is the polysulphide of sodium, which in green ultramarine is $Na S^2$, and in blue $Na S^5$. All the properties of blue and of green ultramarine, as well as their formation, justify this mode of the arrangement of the sulphur.

The sulphuric acid found in the solution after decomposition of ultramarine by hydrochloric acid, is not formed, as was above shown, by the decomposition of a hyposulphite. It is united with lime, and rather hinders the formation of the colour than otherwise.

That the sulphur in the form of a hyposulphite cannot contribute to the constitution of the colour, is evident from the fact, that, at the temperature at which ultramarine is formed, a hyposulphite could not exist, but would be resolved into sulphur and a sulphate. Lastly, besides the oxygen in the silica, the alumina, the oxide of iron, and the soda, there is no more present in ultramarine. The agreement of the analyses shows this. The sulphur can only contribute to the formation of the blue or green colour in the form of polysulphide of sodium.

On bringing hydrochloric acid in contact with ultramarine, one of its chief constituents, the polysulphide of sodium, is decomposed, sulphuretted hydrogen is evolved, and sulphur precipitates. On the addition of excess of concentrated acid, persul-

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phide of hydrogen is evolved, which is recognizable by its odour. The changes which ultramarine undergoes by treatment with hydrogen, by ignition alone, or mixed with nitre and soda, prove sufficiently the presence of polysulphide of sodium.

From the last-named experiment, it is evident that the sulphide in green ultramarine is a lower one than that in blue, for the melted mass becomes first colourless and then white. So likewise green passes into blue when ignited with soda and sulphur in the proportion to form polysulphide of sodium, or with sulphur alone, or heated for itself in the air.

In all these three cases the polysulphide of the blue is formed from the bisulphide of the green ultramarine. In the first two cases the change is effected by the simple taking up of sulphur; in the last case, a part of the silica of the silicate withdraws sodium from the bisulphide, which is oxidized by the air, and polysulphide results.

The Silicate.—This second constituent of ultramarine consists of silica, alumina, and soda. A consideration of the analyses of ultramarine, shows a remarkable agreement in these bodies. I calculated the quantity of oxygen contained in them, and found it to be as follows:—

	Silica.	Alumina.	Soda.
I. . . .	5·1	3·6	1
II. . . .	5·2	2·9	1
III. . . .	4·1	2·7	1
IV. . . .	3·5	2·2	1
V. . . .	4·4	2·6	1
VI. . . .	4·6	2·9	1
VII. . . .	5·9	3·7	1

The quantity of oxygen in the soda, as being present in smallest quantity, was taken as $=1$.

These relations agree best with the composition of a natural silicate, nepheline, in which the oxygen ratios for silica, alumina, and soda, are—

$$4{\cdot}5 : 3{\cdot}0 : 1,$$

for the formula of nepheline is

$$(2NaO)\,SiO^3) + 2(Al^2\,O^3\,SiO^3).$$

In order to ascertain in what equivalent relation sulphide of sodium is united with the silicate, with a view to obtain the formula for chemically pure ultramarine, the oxygen of the soda in the silicate is to be divided by the oxygen contained in the soda corresponding to the sulphide of sodium; there is then obtained:—

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	Oxygen of the soda in the silicate.	Oxygen in the soda corresponding to the sulphide of sodium.	Proportion in round numbers.
I.	3·844	0·992	4 : 1
II.	4·200	1·104	4 : 1
III.	4·962	0·661	7·5 : 1
IV.	5·411	0·736	7·5 : 1
V.	4·438	1·108	4 : 1
VI.	4·369	1·841	2 : 1
VII.	3·582	1·925	2 : 1

Nos. III. and IV. are kinds of a clear pale blue; the others, I., II. and V., are of the finest sorts; it may hence be assumed, that, in chemically pure blue, 1 equivalent of pentasulphide of sodium corresponds to 4 equivalents of soda in the silicate; and in the purest green, 1 equivalent of bisulphide of sodium corresponds to 2 equivalents of soda in the silicate, or in other words,—

In blue, 2 equivs. nepheline with 1 NaS^5,
In green, 1 equiv. nepheline with 1 NaS^2.

The formulæ for the pure compounds are accordingly,—

Blue $2\big((2NaO)SiO^3+2(Al^2O^3\,SiO^3)\big)+1NaS^5$

$1\big((2NaO)SiO^3+2(Al^2O^3\,SiO^3)\big)+1NaS^2$.

In order to compare the values of the constituents of the pure compound as found by analysis, with the per-centages as calculated from the formula, the impurities, oxide of iron, clay, and gypsum, were rejected, and the weights of the bodies constituting the pure compound calculated for 100. For the blue ultramarine the following constitution is obtained:—

	I.	II.	III.	IV.	V.	The formula requires	
Silica	39·7	42·9	40·8	39·2	39·3	38·5	$6SiO^3$
Alumina	31·8	25·4	30·1	27·9	26·9	29·1	$4Al^2O^3$
Soda	15·8	17·1	20·4	22·6	18·5	17·6	$4NaO$
Sodium	3·0	3·3	2·0	2·3	3·4	3·2	$1Na$
Sulphur *a*	2·1	2·3	1·4	1·6	2·4	2·2	$1S$
Sulphur *b*	7·5	8·9	5·2	6·3	9·3	9·1	$4S$
	100·00	100·00	100·00	100·00	100·00		

For the green:—

	VI.	VII.	The formula requires	
Silica	40·3	40·3	38·2	$3SiO^3$
Alumina	28·8	29·4	28·9	$2Al^2O^3$
Soda	17·7	14·4	17·4	$2NaO$
Sodium	5·6	5·8	6·4	$1Na$
Sulphur	7·5	9·9	9·0	$2S$

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That the values as found by analysis do not agree better with the values as calculated from the formulæ, that is, that the constitution of the pure blue and green compound in the ultramarines analysed is not quite the same, arises from the mode of formation, in which the temperature and the quality of the clay exercise great influence. Perhaps the mode in which the analyses were executed is partly at fault. It is possible that the clays used for the preparation exhibit various degrees of resistance to the action of acid and alkali.

The most beautiful blue, that is, the pure compound, will probably be obtained by heating a silicate of the constitution of nepheline mixed with polysulphide of sodium in given proportions, to the right temperature. Strangely enough, according to present experience, green ultramarine is alone formed. If this be finely powdered, and freed by washing from substances soluble in water, as sulphide of sodium, sulphite and hyposulphite of soda, and then heated under access of air, and with the addition of 2 equivs. of sulphur to 2 equivs. of the green compound, 1 equiv. of the blue compound is obtained, and 1 equiv. of sulphate of soda, which is separated by washing. Experience can alone teach at what temperature the green compound is formed, and at what it passes into the blue. In the last operation, access of air, as theory and experiment sufficiently show, is essential. The blue compound of ultramarine is, in all its properties and analyses, the same as that in lapis-lazuli, and in the related minerals, hauyne, nosean, and sodalite.

Unfortunately, of lapis-lazuli no analysis exists capable of a probable interpretation; the last one made by Field corresponds in some measure to the formula

$$3[2(NaO\,SiO^3) + (Al^2O^3 + 2SiO^3)] + NaS.$$

But the sulphur has certainly not been all found.

The formulæ for nosean, hauyne, and sodalite are,—

Nosean	$[(3RO)SiO^3] + 3(Al^2O^3\,SiO^3)$	+	$NaO\,SO^3$
Hauyne	...	...	$+2CaO\,SO^3$
Sodalite	...	...	+ Na Cl

On comparing the silicates of these three minerals, a considerable similarity is not to be disputed; for the relation of the oxygen of the soda to that of the alumina and silica is as 1 : 3 : 4, while the average relation in ultramarine is as 1 : 3 : 4·5. We might hence assume, that in ultramarine the same silicate is contained as in nosean, &c., but in that case we must suppose that the caustic alkali used in the separation of silica and clay had extracted more silica from the residual clay than arose from the decomposed blue compound.

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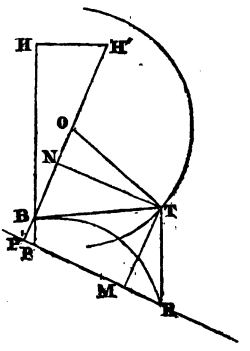
LXIX. *A general Construction for finding the maximum Range of Projectiles* in vacuo. *By the* Rev. JOSEPH A. GALBRAITH, *M.A., Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin**.

IN the last Number of the Philosophical Magazine I read with much pleasure a communication from Professor Sylvester, in which he investigates the maximum range of a projectile on an inclined plane, the point of projection being situated above the plane. At the end of his article he gives a geometrical construction for finding the elevation for the best range on a horizontal plane.

The following investigation, which gives a general construction for planes not horizontal, may prove interesting to those readers of the Philosophical Magazine who feel pleasure in following a train of reasoning strictly geometrical in all its steps. The construction I propose is very easily effected, so that if drawn to scale it is capable of furnishing very good practical results. Moreover, it may be said from its generality to command every case which may be proposed with regard to projectiles *in vacuo*.

Let B be the battery from which the shot is fired, at a given vertical height BP above the descending plane PR; let PR be the range of the ball, BT the direction of the gun, and RT a vertical line drawn through R to meet this line in T; then the *locus* of the point T for a given velocity of projection shall be a circle. For cut off BH equal to $4h$, or four times the height due to v, the velocity of projection, and draw HH′ horizontal to meet in H′ the perpendicular BP′, let fall on the plane, bisect BH′ in O and join OT; from T draw TM and TN perpendicular and parallel to the plane.



Let t be the time of flight; then $BT=vt$, and $TR=\frac{1}{2}gT^2$. Eliminating t, and substituting for v^2 its equal $2gh$, we obtain

$$BT^2=4h\times TR=BH\times TR.$$

Since the triangles HBH′ and MTR are similar,

$$BH\times TR=BH'\times TM;$$

therefore $BT^2=BH'\times TM$. In the triangle OTB,

$$OT^2+OB^2-2OB\,.\,ON=BT^2=BH'\,.\,TM;$$

* Communicated by the Author.

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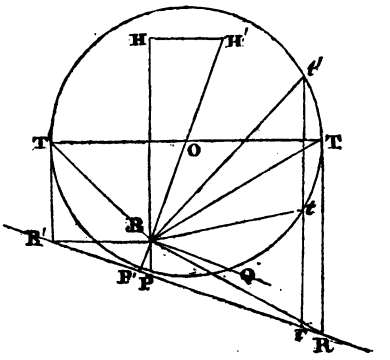
therefore

$$\begin{aligned} OT^2 &= OB^2 + BH' \cdot TM + 2OB \cdot ON - 2OB^2 \\ &= OB^2 + BH'(TM + ON - OB) \\ &= OB^2 + BH' \cdot BP'. \end{aligned}$$

From this equation it is evident that OT is given in length, and therefore that for a given velocity of projection the locus of T is a circle. The centre and radius of this circle may be found by the following construction:—

Draw through B the lines PBH vertical, and P'BH' perpendicular to the plane; cut off BH $= 4h$, and draw HH' horizontal; bisect BH' in O; O is the centre of the circle. To find the radius, draw BQ parallel to the plane PR; on P'H' describe a semicircle, cutting this line in Q; the distance OQ is the radius of the circle.



If the range be given $= Pr$ suppose, draw through r a vertical line cutting the circle in the points t and t'; Bt and Bt' will be the *two* directions of the gun answering to this range. If the *maximum* range be sought, it is evident that the vertical through the extremity of the range must touch the circle; therefore, if the horizontal line T'T be drawn through O, cutting the circle in T' and T, the lines BT and BT' will be the directions of the gun for the best range, the one down, the other up the plane.

The *isosceles* of the triangle BRT may be proved as follows:— Imagine the line OR drawn in the figure, then

$$\begin{aligned} OR^2 - RB^2 = OP'^2 - BP'^2 &= (OP' + BP')(OP' - BP') \\ &= (OB + 2BP') \cdot OB \\ &= OB^2 + 2OB \cdot BP' \\ &= OB^2 + BQ^2. \end{aligned}$$

But

$$OB^2 + BQ^2 = OQ^2 = OT^2 = OR^2 - RT^2.$$

Therefore

$$OR^2 - RB^2 = OR^2 - RT^2.$$

Therefore

$$RB = RT.$$

Similarly

$$R'B = R'T'.$$

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In the case supposed by Professor Sylvester, *i. e.* $v=1200$ feet, and the height BP = 800 feet, the line BH, if we suppose $g=32$, will be 800 times BP, in which case the point B will lie so near the circumference that the line BT will practically make equal angles with the plane and the vertical. So that, in general, the fact of the ball being fired from a height above a plane should make no difference as to pointing the gun for a maximum range on that plane.

LXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 477.]

June 21, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"Report of a Committee appointed by the Council to examine the Calculating machine of M. Scheutz*."

The various applications of mathematics to physical questions, or to the transactions of common life, continually require the computation of numerical results. At one time isolated results have to be calculated from particular formulæ; at another it is required to calculate a series of values of the same analytical formula; in other words, to tabulate a function. It is only in the latter case that different instances have so much in common as to permit of the application of general methods irrespective of the particular function to be calculated. But even in the tabulating of functions one or other of two objects may be kept in view. At one time a result may be arrived at expressed in a complicated, perhaps transcendental, formula, and the mathematician may desire to know merely the general progress of the function. In such a case it will be sufficient to calculate values at rather wide intervals, and the mode of calculation must depend upon the peculiar function. But at other times functions present themselves which are of such common occurrence, or of such practical importance, that it is desirable to tabulate them for values of the variable increasing by small steps. In these cases general methods of interpolation come into use: it is sufficient to perform the calculations directly for comparatively wide intervals of the variable, and the intervening values of the function can be supplied by the mere addition of differences.

It is well known that Mr. Babbage was the first person who conceived the idea of performing all these systems of additions mechanically, and thereby saving both the mental labour and the risk of error attending their calculation in the ordinary way. This idea was actually carried out, and resulted in the invention of his Difference Engine. The engine, so far as it has yet been executed, was constructed at the public expense, and is now deposited in the Museum of King's

* The Committee consisted of Prof. Stokes, Sec. R.S., Prof. W. H. Miller, Prof. Wheatstone, and the Rev. Prof. Willis.

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College, London. The part constructed contains 19 digits and 3 orders of differences; and as all the essential movements are comprised in this part, a more extended engine would consist merely of the same members oftener repeated, and would not involve any additional difficulty of construction. It was part of Mr. Babbage's original design that machinery for printing off the results calculated should be included in his engine, and some of the mechanism for this purpose was actually executed. The portion placed in King's College contains machinery for calculating only. It does not fall within the province of this report to do more than mention the Analytical Engine subsequently invented by Mr. Babbage, as the machine of M. Scheutz is a Difference Engine, and nothing more.

A full account of the principles and action of Mr. Babbage's Difference Engine, but without any details of its mechanism, was published in the 'Edinburgh Review' for April to July, 1834. It was, as we are informed, the perusal of this paper which induced M. Scheutz to set about the invention of modes of mechanically executing the necessary changes. The result was the completion of the present engine, which has now for some time been in the apartments of the Royal Society. In this machine M. Scheutz has followed the general ideas of Mr. Babbage in the distribution of digits and differences, and in particular in throwing back the differences at every alternate order one stage, from whence results the possibility of acting simultaneously on all the odd and on all the even differences, and thereby making the machine advance one stage by two addition-motions only; whereas otherwise as many separate addition-motions would have been necessary as there were orders of differences retained. But the mechanism by which the additions and carriages are effected in the machine of M. Scheutz is different from that of Mr. Babbage. The engine is also provided with mechanism for printing, or rather for furnishing stereotype plates of the calculated results.

As M. Scheutz has taken out a patent for his engine, it will be unnecessary to give a detailed description of the machinery, which may be obtained in the specification, a copy of which has been presented to the Royal Society. It will be sufficient to give an idea of its general construction and extent with a view of estimating its powers.

The machine takes in the function to be tabulated and the first four orders of differences, each to fifteen digits. Of these only the first eight (in the case of the function itself) are printed, the others being reserved to guard against errors arising from decimal places left out.

The places of the digits are represented by fifteen vertical spindles, around which, but not usually connected with which, are placed horizontal wheels in five separate tiers. Each wheel has its circumference divided into ten equal parts, and is marked with the digits 0, 1, 2, 3, 4, 5, 6, 7, 8, 9. In the normal state of the machine the numbers on the wheels of the highest tier represent the function (u_x) to be tabulated, and those on the tiers below represent respect-

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ively Δu_{x-1}, $\Delta^2 u_{x-1}$, $\Delta^3 u_{x-2}$, and $\Delta^4 u_{x-2}$. In each case the digits by which these numbers are represented run from left to right, as in print. The mechanism is such, that by turning a handle continuously in one direction an indefinite succession of movements is produced which are alternately backwards and forwards. The effect of the forward motion is, that the numbers on the third and the fifth tiers (or as they may conveniently be called the Δ^2 and Δ^4 tiers) add themselves respectively to those on the tiers above, altering thereby the positions of the wheels of the Δ^1 and Δ^3 tiers, while the wheels of the Δ^2 and Δ^4 tiers remain at rest; and the backward motion does for the Δ^1 and Δ^3 tiers what the forward motion does for the Δ^2 and Δ^4 tiers. Thus the numbers on the several tiers will be as follows:—

At first	u_x	Δu_{x-1}	$\Delta^2 u_{x-1}$	$\Delta^3 u_{x-2}$	$\Delta^4 u_{x-2}$;
After the forward motion	u_x	Δu_x	$\Delta^2 u_{x-1}$	$\Delta^3 u_{x-1}$	$\Delta^4 u_{x-2}$;
After the complete motion ..	u_{x+1}	Δu_x	$\Delta^2 u_x$	$\Delta^3 u_{x-1}$	$\Delta^4 u_{x-1}$,

$\Delta^4 u_{x-1}$ in the last term being written instead of $\Delta^4 u_{x-2}$, which is allowable, since the fourth differences are supposed to be constant. Hence the effect of the complete motion, consisting of one forward and one backward motion, is to make all the numbers advance one stage; and therefore by continuing to turn the handle the numbers u_{x+1}, u_{x+2}, u_{x+3} &c., will be calculated in succession. According as these numbers are calculated they are impressed, by the action of the machine itself, on a plate of lead, by means of steel punches, while a numerator at the same time impresses beside them the values of the argument x. These plates are afterwards taken out, and stamped on an easily fusible alloy just on the point of solidifying, and thus are obtained stereotype plates of the calculated results, fit for printing from.

In retaining a given number of decimals, it is usual to add one to the last figure if the first digit left out be 5 or a higher number. This is effected in the machine in the simplest possible manner, namely by placing the cog which occasions the carriages from the ninth to the eighth place in the highest tier in such a position that the carriage takes place when the ninth wheel changes from 4 to 5, instead of from 9 to 0.

The principle of the machine is not of course dependent upon the circumstance that the radix of the scale of notation commonly employed has the particular value 10; and it would be as easy to construct a machine adapted to the senary or duodenary as to the denary scale. Not only so, but the machine actually constructed admits of being changed very readily from the denary to the senary scale, or rather to a mixture of the denary and senary scales, which is required in tabulating degrees, minutes, and seconds. For this purpose it is sufficient to take off the ordinary figure-wheels from those spindles which are to count by sixes, and put on spare wheels which are provided, adapted to the senary scale.

The machine works with the greatest freedom and smoothness.

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The parts move with the utmost facility, in fact, quite loosely. On this account no amount of dust which it would reasonably be expected to receive in any moderate time seems likely to interfere with its action. Besides, it can easily be taken to pieces and examined, if need be. Those motions which are not the direct consequences of the revolution of the handle acting through a train of rigid bodies are performed in consequence of gravity, no springs being employed in the whole construction except two, the office of which is quite subordinate. When the parts are moved, they remain in their new places either from their weight or from friction, there being nothing to disturb them. This circumstance, which renders a wilful derangement of the machine exceedingly easy, permits of great simplicity and consequent cheapness of construction; nor does the machine seem likely to get out of order if reasonable care be taken of it.

The machine is competent to tabulate to any extent a function whose fourth differences are constant, so long as the expression of the numerical value of the function does not involve more than eight digits. The most general form of such a function is of course

$$a+bx+cx^2+dx^3. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Were the machine restricted to such functions, its use would be limited indeed; its utility must of course depend on its being applicable to functions in general, which, except in singular cases, may be expressed within a limited range of values of the variable x by a function of the above form. To estimate the capacities of the machine, or rather of a difference engine in general, whatever may be its particular construction, it will be necessary to investigate how soon the quantities neglected begin to tell in the result.

Now these quantities are of two kinds; first, the fifth and higher differences; secondly, the decimals of the fifteenth place. The effect of these may be examined separately. We may always suppose the first spindle to represent the first place of decimals, since it will only be necessary to multiply or divide by some power of 10 should that not be the case.

Suppose the machine set for u_x, and its first four differences (or to speak more exactly, the differences Δu_{x-1}, $\Delta^2 u_{x-1}$, $\Delta^3 u_{x-2}$, $\Delta^4 u_{x-2}$), and worked n periods, so as to give what ought to be u_{x+n}. We have

$$u_{x+n}=u_x+\frac{n}{1}\Delta u_x+\frac{n.\overline{n-1}}{1.2}\Delta^2 u_x+\ldots. \quad . \quad . \quad . \quad (2);$$

and since the machine would give u_{x+n} exactly if the fourth differences were constant, the error (E) will be

$$\frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}.\overline{n-4}}{1.2.3.4.5}\Delta^5 u_x+\frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}.\overline{n-4}.\overline{n-5}}{1.2.3.4.5.6}\Delta^6 u_x+\ldots.*$$

* This expression will not be absolutely exact, since it is Δu_{x-1}, $\Delta^2 u_{x-1}$, $\Delta^3 u_{x-2}$, $\Delta^4 u_{x-2}$, and not Δu_x, $\Delta^2 u_x$, $\Delta^3 u_x$, $\Delta^4 u_x$ that are given correctly; but the inaccuracy thus arising in the estimation of the error committed by leaving out the fifth, &c. differences will plainly be insignificant.

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The first term in this expression will usually be the most important; and for practical purposes the expression may be still further simplified. If n be tolerably large, the product $n \cdot \overline{n-1} \cdot \overline{n-2} \cdot \overline{n-3} \cdot \overline{n-4}$ may be replaced without material error by the fifth power of the arithmetic mean of the factors, or by $(n-2)^5$. Again, if y be the variable of which u is a function, x being merely the numeral marking the number of increments of y, each equal to k, we shall have near enough

$$\Delta^5 u_x = k^5 \frac{d^5 u}{dy^5},$$

so that

$$E = \frac{1}{120}(n-2)^5 k^5 \frac{d^5 u}{dy^5}.$$

In expressing a number to eight decimal places, we are always liable to an error which may amount to 5 in the ninth place. Hence $10^{-9} \times 5$ may be regarded as the greatest allowable error, though in truth the error should not be allowed to amount to this, if we wish to have the last figure true to the nearest decimal. Equating then E to $10^{-9} \times 5$, we find

$$n = 2 + \left(\frac{\cdot 0000006}{\frac{d^5 u}{dx^5}} \right)^{\frac{1}{5}} \cdot \frac{1}{k}, \quad \ldots \ldots \quad (3)$$

which gives the greatest number n of times the machine may be worked without stopping and fresh setting, so far as the limitation depends on the cause of error now under consideration. The increment of y during the action of the machine, which is equal to nk, or to $(n-2)k$ nearly, n being large compared with 2, is therefore nearly independent of the closeness or wideness of the intervals for which the value of the function is required, a given range, so to speak, of the function being taken in. Hence, so far as this cause of limitation is concerned, the utility of the machine will be proportional to the closeness of the intervals for which it is desired to tabulate the function.

Let us now consider the effect of the decimals omitted, retaining only four orders of differences, since the effect of omitting the fifth and higher orders has been already investigated. Let E_1, E_2, E_3, E_4 be the errors left in the first, second, third, and fourth differences in setting the machine. Then in the same manner as before these may without sensible error be regarded as the errors in Δu_x, $\Delta^2 u_x$, $\Delta^3 u_x$, $\Delta^4 u_x$, although they are really the errors in Δu_{x-1}, &c., and we shall have for the error (E) in u_{x+n}

$$E = \frac{n}{1} E_1 + \frac{n \cdot \overline{n-1}}{1.2} E_2 + \frac{n \cdot \overline{n-1} \cdot \overline{n-2}}{1.2.3} E_3 + \frac{n \cdot \overline{n-1} \cdot \overline{n-2} \cdot \overline{n-3}}{1.2.3.4} E_4;$$

or, replacing the products as before,

$$E = nE_1 + \frac{1}{2}\left(n - \frac{1}{2}\right)^2 E_2 + \frac{1}{6}(n-1)^3 E_3 + \frac{1}{24}\left(n - \frac{3}{2}\right)^4 E_4. \quad \ldots \quad (4)$$

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If each of the quantities E_1, E_2, E_3, E_4 be liable to be as great as $10^{-16} \times 5$, the last term in this expression will be the most important if n be considerably greater than 4. Equating this term to $10^{-9} \times 5$, the greatest allowable error in E, we find

$$n - \frac{3}{2} = (24 \times 10^7)^{\frac{1}{4}}, \qquad n = 126 \text{ nearly},$$

so that the machine may be worked about 100 times without fresh setting.

In practice the limitation may be even less than this; for it may happen that $\Delta^4 u_x$ is smaller, perhaps much smaller, than $10^{-16} \times 5$, in which case the limitation will depend upon the absolute value of $\Delta^4 u_x$ or the possible value $10^{-16} \times 5$ of E_3, as the case may be. Should the restriction arise from the latter cause, we get by equating the third term in the second member of (4) to $10^{-9} \times 5$, $n = 392$ nearly.

To illustrate these limitations by an example, suppose that it was required to make a table of sines to every minute. In this case we have

$$u = \sin y, \quad k = \frac{\pi}{180 \times 60} = \cdot 0002909, \quad \frac{d^5 u}{dy^5} = \cos y.$$

Putting for this last differential coefficient its greatest value unity, and substituting in (3), we get $n = 196$ nearly. The fourth difference is very nearly equal to $-k^4 \sin y$, which may contain figures in the fifteenth place, so that $n = 126$ is about the greatest allowable value of n in consequence of the restriction arising from decimals left out, which in this example is what limits the working. Should the intervals be a good deal wider than $1'$, as $5'$, it would then be the omission of fifth differences that would impose the limit, for the greatest allowable range on this account would be nearly the same as before, or about $3°$, which would contain only thirty-six values to be calculated. Should it happen that both causes of error were about equally restrictive, it must be remembered that the corresponding errors in u_x would be comparable with one another, and might be added together; and in this case it may easily be shown that $126 \times 2^{-\frac{1}{4}}$, or 106 nearly, is somewhat inferior to the greatest allowable value of n. Should eight figures not be required to be retained, but seven, six, or five be sufficient, the last one, two, or three of the first eight spindles might be used for calculating instead of printing; and since the greatest allowable value of n, so far as depends on omission of decimals, varies nearly as the fourth root of the greatest allowable error in u_x, that value would be increased in the ratio of 1 to the fourth root of 10, or 100, or 1000, and from 126 would become 224, or 398, or 708. The greatest allowable value of n as regards the omission of fifth differences would increase in a somewhat slower ratio, since it varies nearly as

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the fifth root of the greatest allowable error in u_x. If, for example, it were 196, it would become 311, or 492, or 780.

The above is a fair specimen of the application of the machine. The particular function chosen is, it is true, a familiar one, which has been long since tabulated, but it is not the worse fitted for an example on that account. It may be seen at once how much mental labour and risk of error is saved by the use of such a machine, when tables have to be calculated to close intervals. The whole exertion of mind is confined to calculating the function and its differences at wide intervals, say for every 100th or 60th number to be tabulated, and setting the machine. Even this exertion (except so far as relates to the setting, which is easy,) might be reduced to one half, if desired, by setting the machine to calculate backwards as well as forwards. In order to give in succession the numbers u_x, u_{x+1}, u_{x+2}, ... the machine has to be set to

$$u_x \quad \Delta u_{x-1} \quad \Delta^2 u_{x-1} \quad \Delta^3 u_{x-2} \quad \Delta^4 u_{x-2},$$

or to

$$u_x \quad \Delta D^{-1} u_x \quad \Delta^2 D^{-1} u_x \quad \Delta^3 D^{-2} u_x \quad \Delta^4 D^{-2} u_x,$$

D denoting as usual the operation $1+\Delta$. In order to give in succession the numbers u_x, u_{x-1}, u_{x-2}, ... the machine would simply have to be set to

$$u_x \quad \Delta' D'^{-1} u_x \quad \Delta'^2 D'^{-1} u_x \quad \Delta'^3 D'^{-2} u_x \quad \Delta'^4 D'^{-2} u_x,$$

if $D'u_x$ be used to denote u_{x-1}, and Δ' to denote $D'-1$. But $D'=D^{-1}$, and $\Delta'=D'-1=D^{-1}-1=-D^{-1}\Delta$, so that the required numbers are

$$u_x \quad -\Delta u_x \quad \Delta^2 D^{-1} u_x \quad -\Delta^3 D^{-1} u_x \quad \Delta^4 D^{-2} u_x,$$

or

$$u_x \quad -\Delta u_x \quad \Delta^2 u_{x-1} \quad -\Delta^3 u_{x-1} \quad \Delta^4 u_{x-2}.$$

Hence the numbers on the top, Δ^2, and Δ^4 tiers are the same as for the forward calculation, while those on the Δ and Δ^3 tiers are the arithmetical complements of the numbers found on those tiers after the machine has made one complete movement in calculating forwards from u_x. The printing part, however, is not adapted to such a change: the numbers would be printed off correctly, but in a wrong order; so that unless some reversing movement were introduced into the printing part, the printed results would only serve to set types from.

In the example chosen above, and in similar cases, the differences required for setting the machine would be calculated from their mathematical expressions. It might, however, be required to tabulate for small intervals a function which had been given by observation for larger ones, or to tabulate a mathematical function of so complicated a form that the differences could not be got directly without great trouble. In such a case there would be no difficulty;

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the differences for the smaller intervals would first have to be calculated from those for the larger ones by formulæ in finite differences, and then the setting and working of the machine would proceed as before.

It must be confessed, however, that except in the case of mathematical tables like those of sines, cosines, logarithms, &c., it is not ordinarily required to tabulate functions to intervals at all approaching, in closeness, to those in the example selected. Hence it is mainly, as it seems to us, in the computation of mathematical tables that the machine of M. Scheutz would come into use. The most important of such tables have long since been calculated; but various others could be suggested which it might be worth while to construct, could it be done with such ease and cheapness as would be afforded by the use of the machine. It has been suggested to us too, and we think with good reason, that the machine would be very useful even for the mere reprinting of old tables, because it could calculate and print more quickly than a good compositor could set the types, and that without risk of error.

G. G. Stokes.
W. H. Miller.
C. Wheatstone.
R. Willis.

P.S. Some time since, I received from Mr. Babbage, to whom I had written for information on one point connected with his machine, a letter, written subsequently to his first answer, in which he said that he had forgotten to mention an addition to his machine which enabled it to calculate a function when the last differences, instead of being constant, were dependent on the functions then under calculation in the other parts of the machine, provided the coefficients of the variable part were small enough to be expressed by a moderate number of digits. This was especially designed for the calculation of astronomical tables, where a difficulty occurs in the application of a machine with constant differences, arising from the circumstance that in the case of functions of short period the omitted differences soon become sensible even though the coefficients be but small. Mr. Babbage did not then recollect that this contrivance was accessible to the public, but in a subsequent letter he pointed out that such was the case. The following is an extract from this letter:—

"1st. The portion at Somerset House contains axes specially prepared for what (at this instant) I recollect to have familiarly called 'eating its own tail.'

"2nd. The drawings contain the modes of governing those axes in the finished engine.

"These are public property, and open daily to public inspection, which I suppose must be considered as publication. On referring to the 9th Bridgewater Treatise, second edition, I find (p. 34) that I have used as an illustration a series computed by that very machine. * * *."

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In the same letter Mr. Babbage refers to the following documents:—

Extract from a letter of Mr. Babbage to Sir H. Davy, 3 July, 1822, printed by order of the House of Commons. No. 370, 1823:—

"Another machine, whose plans are more advanced than several of those just named, is one for constructing tables which have no order of differences constant (p. 2).

"I should be unwilling to terminate this letter without noticing another class of tables of the greatest importance, almost the whole of which are capable of being calculated by the method of differences. I refer to all astronomical tables for calculating the places of the sun and planets. It is scarcely necessary to observe that the constituent parts of these are of the form $\alpha \sin \theta$." (p. 5.)

He refers also to an extract from the Address of H. T. Colbroke, Esq., President of the Astronomical Society, on presenting to him the first medal given by the Society, 1824; and to a description of his machine by the late Mr. Baily, published in Schumacher's 'Astronomische Nachrichten,' No. 46, and republished in the 'Philosophical Magazine' for May 1824, p. 355. This last paper describes fully what could be done by the new contrivance.

I have ventured to insert this postscript without consulting my colleagues, as it is desirable not to delay the publication.

G. G. Stokes.

London, Oct. 5, 1855.

December 6.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

The following communication was read:—

"On the Representation of Polyhedra." By the Rev. Thomas P. Kirkman, A.M.

This paper constituted an addition to the paper by the same author read June 21, 1855.

The author observes that to every p-acral q-edron corresponds a p-dral q-acron, the summits and faces of either having the same order and rank as to the number of edges with the faces and summits of the other. When $p=q$, the corresponding pair will sometimes be identical figures, as to the number, rank, and arrangement of faces and summits; and at other times, as is always the case if p be not equal to q, the two figures will differ. When they differ they may be called a *sympolar* pair, both being *heteropolar*; when they form one and the same figure it may be styled an *autopolar* polyhedron. An elegant way of representing a sympolar pair is deduced from the two following theorems:—

A. The q summits of a q-acron are the angles of a closed polygon of q sides, all edges of the q-acron.

B. A closed polygon of p sides can be traced on the p faces of every p-edron, having a side in every face, and passing through no summit.

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Dec. 13.—Colonel Sabine, R.A., Treas. and V.P., in the Chair.

The following communication was read:—

"On the Action of Sulphuric Acid on the Nitriles and on the Amides." By G. B. Buckton and A. W. Hofmann, Ph.D., F.R.S.

Although the identity of the nitriles with the hydrocyanic ethers has been established by the experiments of M. Dumas, who obtained them by the action of anhydrous phosphoric acid upon certain ammoniacal salts, chemists have hitherto in vain sought for a method by which a passage might be effected from the nitriles to the general alcohol derivatives.

Our attention has been directed likewise to the same subject, but our exertions, like those of our predecessors, have not yet been crowned with success. The researches on which we have been engaged, have, however, furnished some results which appear to us worthy of the interest of chemists, the reactions being at the same time remarkable for their neatness and susceptibility of general application.

Acetonitrile may be considered as the type of its class, both from the importance of the group to which it belongs, and the facility of its preparation. It is, in fact, the nitrile with which we have been specially engaged. This body when mixed with its own volume of fuming sulphuric acid, evolves a considerable amount of heat, accompanied by a very energetic reaction.

If care be taken to add the acid in small quantities, and to cool the mixture after each addition, scarcely any change of colour is to be observed. By the addition of water, and subsequent saturation with carbonate of barium, a crystalline salt is produced in considerable quantity, which possesses all the characters and composition of the sulphacetate of barium,

$$C_4 (H_2 Ba_2) O_4 2SO_3,$$

discovered some time ago by M. Melsens in the mutual reaction of anhydrous sulphuric acid upon glacial acetic acid.

If, on the contrary, the mixture of acetonitrile and fuming sulphuric acid be made rapidly, and the liquid be rather strongly heated, an abundant evolution of carbonic acid indicates a more profound reaction. The residuary mass, which is tough and of a resinous consistency, when treated with water, and boiled with excess of carbonate of barium, yields a magnificent crystallization of a salt represented by the formula

$$C_2 (H_2 Ba_2) 4SO_3 + 4aq.$$

It is a substance of remarkable stability. It does not lose weight at a temperature of 100° C., but four equivalents of water of crystallization are disengaged at 170° C. It is not further affected by a temperature of 220° C. A strong heat resolves it into water, sulphide of barium, sulphurous acid, free sulphur, and carbonic oxide. It may be boiled for hours with fuming nitric acid without the slightest decomposition.

We have also made analyses of the ammonia and silver salts. The former crystallizes with great facility in colourless oblique

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prisms, which may be readily obtained upwards of an inch in length. They are anhydrous, and perfectly stable at a temperature of 190° C.

The composition of this salt is represented by the formula

$$C_2\,[H_2\,(NH_4)_2]\,4SO_3.$$

The silver salt is obtained by digesting oxide of silver with an aqueous solution of the new acid. It forms large crystals, which are easily soluble in water, but insoluble in alcohol or ether. They contain

$$C_2\,(H_2\,Ag_2)\,4SO_3.$$

The acid may be obtained by decomposing either the lead or silver salt by hydrosulphuric acid, or perhaps more conveniently, by carefully precipitating a solution of the barium salt with sulphuric acid. It is an exceedingly soluble and deliquescent substance, crystallizing in long needles. It has a sharp acid taste, with somewhat of the flavour of tartaric acid.

For want of a more convenient name, we propose for this acid the appellation of Methylo-tetra-sulphuric acid. Without wishing to decide at present upon its constitution, its composition allows us to consider it as formed by the association of marsh gas with four equivalents of anhydrous sulphuric acid.

In the reaction of sulphuric acid upon acetonitrile, two distinct phases may be traced. In the first, the nascent acetic acid simply combines with two equivalents of sulphuric acid; in the second phase, the acetic molecule undergoes a more thorough transformation; faithful to its tradition it splits into carbonic acid and marsh gas, which remains combined with four equivalents of sulphuric acid. The new substance also may be regarded as sulphacetic acid, which, losing carbonic acid, has assimilated an equal number of equivalents of sulphuric acid.

The two reactions may be represented by the following equations:—

$$\underbrace{C_4H_3N}_{\text{Acetonitrile.}} + 2HO + 3HSO_4 = \underbrace{C_4H_4O_4\,2SO_3}_{\text{Sulphacetic acid.}} + NH_4SO_4.$$

$$\underbrace{C_4H_3N}_{\text{Acetonitrile.}} + 5HSO_4 = \underbrace{C_2H_4\,4SO_3}_{\text{Methylo-tetra-sulphuric acid.}} + NH_4SO_4 + 2CO_2.$$

The action, then, of bases and of acids upon acetic acid presents a remarkable analogy.

The nature of the change which the acetic molecule suffers, is in fact identical. Under the influence of both agents, it splits into marsh gas and carbonic acid, but in the former case it is the carbonic acid which is fixed, whilst in the latter it is the marsh gas that remains in combination.

The production of methylo-tetra-sulphuric acid calls to mind the interesting substance, sulphate of carbyle, discovered by M. Magnus, by combining olefiant gas with the vapour of anhydrous sulphuric acid. Our new acid is however easily distinguishable from this body, as well by its different composition as by its extreme stability;

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sulphate of carbyle, as is well known, being decomposed by water into isæthionic acid.

As acetamide differs from acetonitrile only in containing two additional equivalents of water, it undergoes with Nordhausen sulphuric acid a strictly analogous transformation.

From the comparative facility of its preparation it offers peculiar advantages for procuring the methylo-tetra-sulphates. The only difference to be noted is, that in this case the ammonia salt is generally eliminated instead of the free acid.

M. Melsens, in his researches upon the sulphacetates, appears in some sort to have anticipated the existence of the methylo-tetra-sulphates. He remarks that he once found in the mother liquor obtained from the preparation of sulphacetate of silver, a crystalline salt, the composition of which he represents by the formula

$$C_2 H_2 Ag_2 S_4 O_{12}.$$

It is evident that these crystals contain the same elements as methylo-tetra-sulphate of silver, but M. Melsens does not appear to have investigated the subject further than by showing the existence of this silver salt.

A detailed description of the methylo-tetra-sulphates, and the study of the corresponding bodies of other series, will be on our part the subject of a special memoir.

GEOLOGICAL SOCIETY.

[Continued from p. 482.]

January 23, 1856.—W. J. Hamilton, Esq., Pres. G.S., in the Chair.

The following communications were read:—

1. "On the Cryolite of Greenland." By J. W. Tayler, Esq. Communicated by J. Tennant, Esq., F.G.S.

At Evigtok, on the shore of the Fiord of Arksut, in South Greenland, is a mass of Cryolite, 80 feet thick, 300 feet long, and dipping southward, at an angle of 45°, between two planes of the including gneiss, to an unknown depth. The upper or roof gneiss is separated from the Cryolite by a thin layer of quartz-crystals, and a rich vein of argentiferous galena, associated with some copper- and iron-pyrites and sparry iron-ore. The same minerals, together with fine crystals of tantalite, are diffused on the upper part of the Cryolite to the depth of a few feet. The mass of Cryolite is quite pure until within 10 feet of the under or floor gneiss, when again similar minerals are disseminated in it. The Cryolite is separated from the under gneiss by a vein of dark purple fluorspar.

The central Cryolite mass, when exposed at the surface (to which the sea has access), is quite white; but, when penetrated to a depth of 10 feet, although free from other minerals, it is of a darker colour, and at 15 feet depth becomes nearly black and more translucent than the outer portion of the mass or bed.

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The author finds that the black-coloured Cryolite, when heated to a dull redness, loses about 1 per cent. of moisture and acid, and becomes less translucent; and he considers that the Cryolite was originally dark-coloured, and that the white and somewhat less compact and more opaque variety has resulted from the action of some external cause; possibly, he thinks, in this case the agency of overlying igneous rocks. Two vertical trap-dykes traverse the cliff near by, one on either side of the Cryolite; and, although no such rocks now overlie the spot in question, the author thinks it quite probable that the known powerful effects of the atmospheric agencies in the Greenland climate may have removed all traces of the overlying eruptive rocks, to the former existence and influence of which he refers the change of condition seen in the superficial Cryolite.

2. "Description of remarkable Mineral Veins." By Professor D. T. Ansted, F.R.S., F.G.S.

This communication was introductory to a series of notices of remarkable mineral veins, which the author has in contemplation, and comprised a notice of the Cobre Lode of Santiago de Cuba. The author commenced his memoir by a brief statement of the sense in which he used the term "mineral vein," together with some remarks on the nature of mineral veins, and the enumeration of various kinds of observations needed in preparing such a report on mineral veins as shall be useful for reference in subsequent investigations for scientific purposes.

The great Cobre lode, which Prof. Ansted selected for his first notice, as being a very exceptional and remarkable vein, has been known for twenty years as the richest copper lode worked for a continuance during that period. It is remarkable for its great magnitude and complication, its extraordinary richness, the high degree of mineralization of the surrounding "country" (or enclosing rock), and the nature of the adjacent rock-masses. The lode is opened in a hill (about 600 feet above the sea), near the town of El Cobre, about eight miles W.N.W. of Santiago de Cuba. It occurs in a large-grained calcareous porphyritic rock, which towards the south forms a mountain-chain, and is associated with basalts and conglomerates. To the north are hard limestones. The general direction of the mountain-range and watershed, and the strike of the porphyries and conglomerates, and of the lode itself, are nearly east and west. The dip of the lode and of the bedded rocks is to the south, but the former is more vertical than the latter. To the east, and probably also to the west, the coast-range is syenitic. The south-eastern portion of Cuba is subject to frequent earthquakes; the central and western portions are free from them.

The outcrop of the Cobre lode, as at present known, extends about a mile, but probably ranges further to the east. Near its eastern known extremity a branch is given off at an angle of about 30°, which goes to the south-west. This branch is, in the author's opinion, continuous with the Santiago copper-lodes, cropping out to the south of the Cobre, which, though yielding some fine ore, have not as yet at all equalled the Cobre lode in productiveness.

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The main lode of the Cobre is cut off to the west by a cross-course, after being heaved by several slides of small amount. The whole workings on the principal vein are limited to a linear extension of 800 yards, and to a breadth of about 200 yards.

The lode appears to have originally afforded superficially a great breadth of ferruginous earthy mineral (gossan) rich with black oxide of copper, together with red oxide and carbonates, to a depth of 16 fathoms; beneath this, solid sulphurets occupied three courses of ore, now being worked. The northernmost is of large size, though variable, and underlies to the south; the middle and the southernmost courses are smaller, and nearly vertical; all three apparently approximate downwards. The "ground" is highly mineralized both between the courses and outside the walls of the two outer courses. The Cobre mine is open at present to the 160 fathom level below adit.

Having thus described the position of the lode, the structure of the "country," and the dimension and contents of the lode, the author next noticed in detail the heaves and cross-courses, the breadth of the "orey ground," and the wonderful timbering and admirable working of the mines; and after some remarks on the occurrence of mundic and gypsum, on the veinstone, the temperature, &c., he recapitulated as follows:—

The western or productive part of the Cobre lode includes three courses of ore, nearly parallel to each other in strike, but gradually approaching downwards. Two of them are unusually large and rich; the middle one is the smallest. The northernmost (on the foot-wall) is affected by small heaves, and all the "orey ground" is terminated by a cross-course to the west. The intervals between the three courses of ore are occupied by a conglomerate or breccia of decomposing porphyry and greenstone, abounding with lime, and passing into a compact whitish-green porphyry. Associated with the courses of ore, the veinstone, and the "country," are large quantities of iron-pyrites; and at a certain considerable depth the veinstone contains gypsum. Regarding the three courses of ore as parts of one great lode, nearly 200 yards wide at its crop, the lode may be said to dip moderately, and the orey portions are chiefly near the hanging wall and the foot-wall, but also extend in bunches and strings into the intervening veinstone and into the enclosing rock, which is highly mineralized. The courses of ore are superficially indicated by a distinct gossan of spongy quartz and iron-oxide, with highly coloured clays, beneath which are oxides, carbonates, and sulphurets of copper, succeeded by sulphurets of copper and iron, the latter gradually preponderating downwards. The "horses" or areas of unproductive ground within the lode and between the courses of ore, consist chiefly of porphyry, like the surrounding rock, and generally mineralized with iron and copper pyrites. The metalliferous deposit, in accordance with the form of the ground, terminates abruptly to the west, where the hill is precipitous, and dies away towards the east. The heaves and cross-courses do not carry ore.

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LXXI. *Intelligence and Miscellaneous Articles.*

ON SOME NEW COLOURING MATTERS.

BY ARTHUR H. CHURCH AND WILLIAM H. PERKIN.

NASCENT hydrogen, acting upon an alcoholic solution of dinitrobenzole or of nitraniline, produces a crimson coloration, due to the formation of a new substance, to which we have given the name *nitrosophenyline*.

This new body presents some remarkable properties. It fuses below 100° C.; is uncrystallizable, and not volatile without decomposition; it dissolves in alcohol with an orange-red tint, and an alcoholic solution containing only ·2 per cent., although perfectly transparent to transmitted light, presents a flame-coloured luminous opacity in reflected light. Nitrosophenyline dissolves in hydrochloric acid, producing an intense crimson colour, which is changed to a yellowish-brown by alkalies and is restored by acids.

The analysis of nitrosophenyline has led us to the formula

$$C_{12}H_6N_2O_2,$$

which may be written thus:—$C_{12}\overset{H_6}{NO_2}N$, and so may be viewed as aniline, in which 1 equiv. of hydrogen is replaced by 1 equiv. of binoxide of nitrogen: the following equation sufficiently explains the formation of nitrosophenyline:—

$$C_{12}H_4 2NO_4 + 8H = C_{12}H_6N_2O_2 + 6HO.$$

We have produced from all the dinitro-compounds we have yet experimented upon, colouring matters similar to nitrosophenyline: the following is a list of such dinitro-compounds:—

1. Dinitrobenzole .... $C_{12}H_4 2NO_4$.
2. Dinitrotoluole..... $C_{14}H_6 2NO_4$.
3. Dinitroxylole..... $C_{16}H_8 2NO_4$.
4. Dinitroxylole..... $C_{18}H_{10} 2NO_4$.
5. Dinitrocymole..... $C_{20}H_{12} 2NO_4$.
6. Dinitronaphthaline.. $C_{20}H_6 2NO_4$.

We have examined minutely the colouring substance produced in the case of dinitronaphthaline. It proves to be perfectly analogous in composition with nitrosophenyline; in properties also it is similar; and from its alcoholic solution it may be obtained in crystals, having a lustre somewhat similar to that of murexide: its formula, as deduced from our analysis, is

$$C_{20}H_8N_2O_2,$$

which we may arrange thus:—$C_{20}\overset{H_8}{NO_2}N$, and so view it as naphthylamine in which 1 equiv. of hydrogen has been replaced by 1 equiv. of binoxide of nitrogen. This substance we term nitrosonaphthyline. It may likewise be obtained by the action of nitrous acid on naphthylamine, or of nitrite of potassium upon the hydrochlorate of naphthylamine: the following equations represent the three processes for its formation:—

1. $C_{20}H_6 2NO_4 + 8H = C_{20}H_8N_2O_2 + 6HO.$
2. $C_{20}H_9N + NO_3 = C_{20}H_8N_2O_2 + HO.$
3. $C_{20}H_{10}N, Cl + KNO_4 = C_{20}H_8N_2O_2 + 2HO + KCl.$

—*From the Proceedings of the Royal Society for Feb.* 21, 1856.

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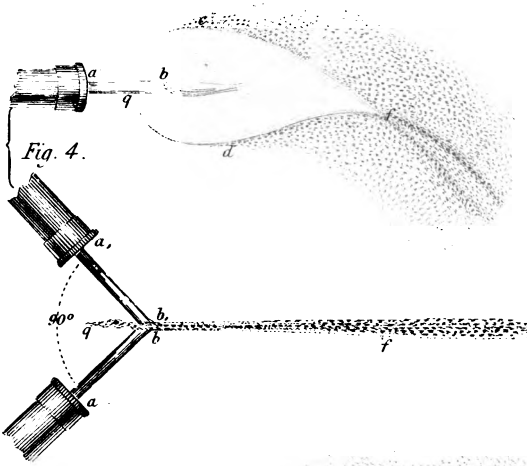

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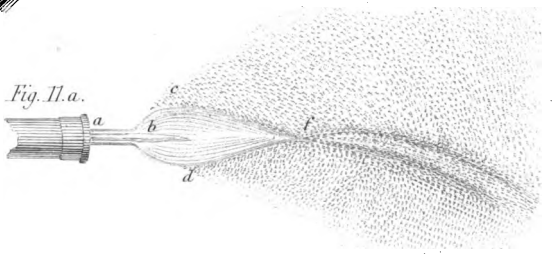

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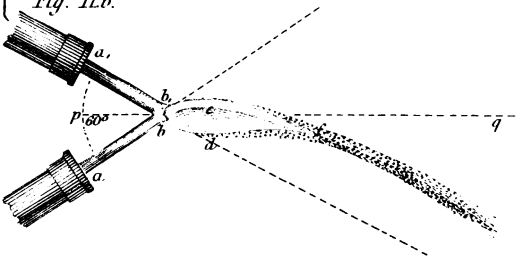

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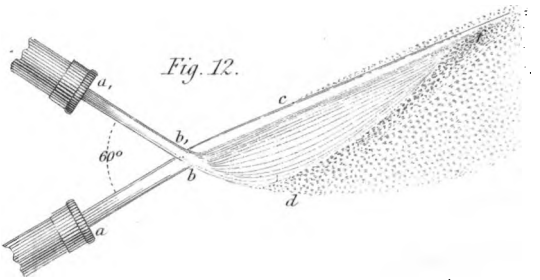

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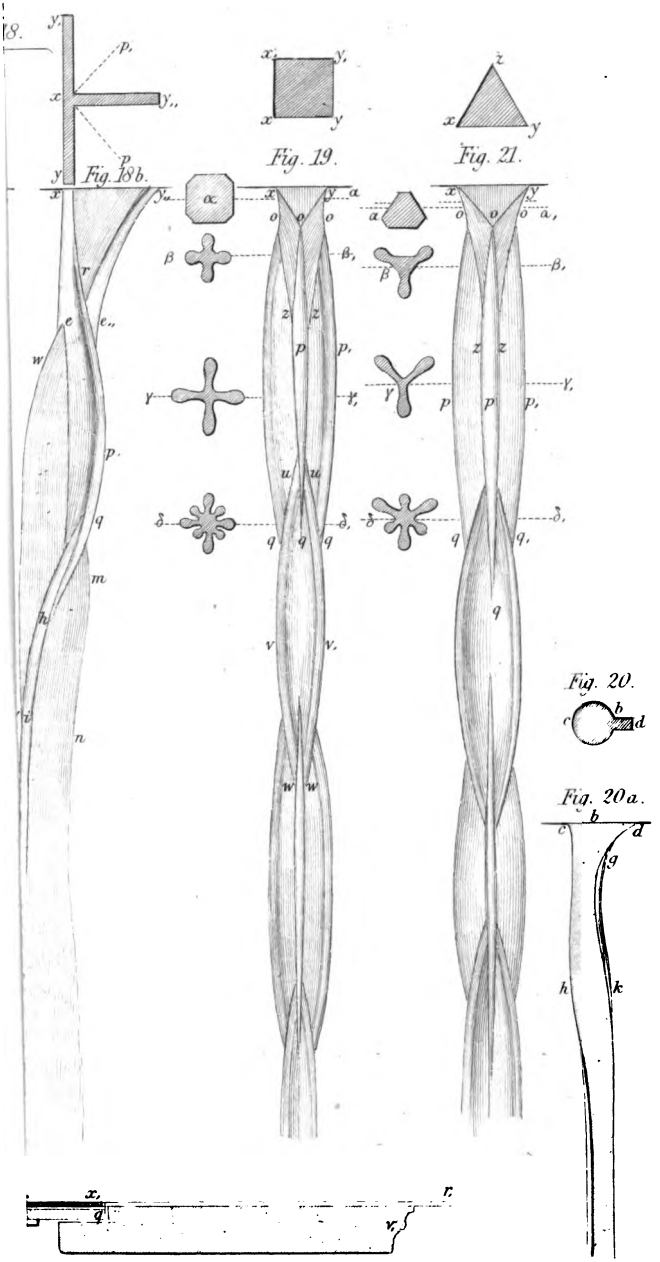

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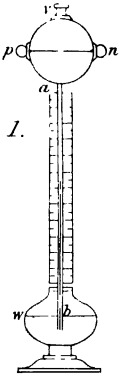

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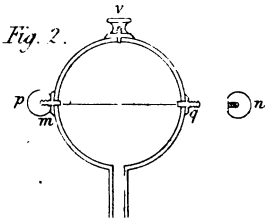

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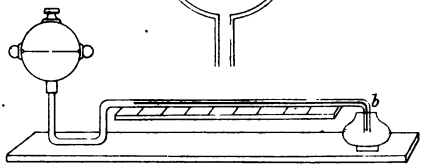

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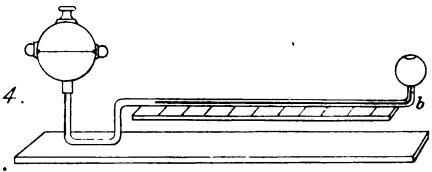

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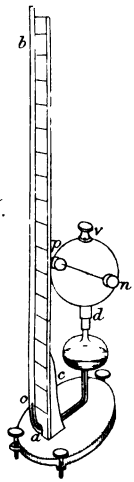

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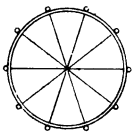

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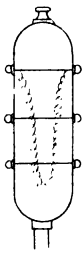

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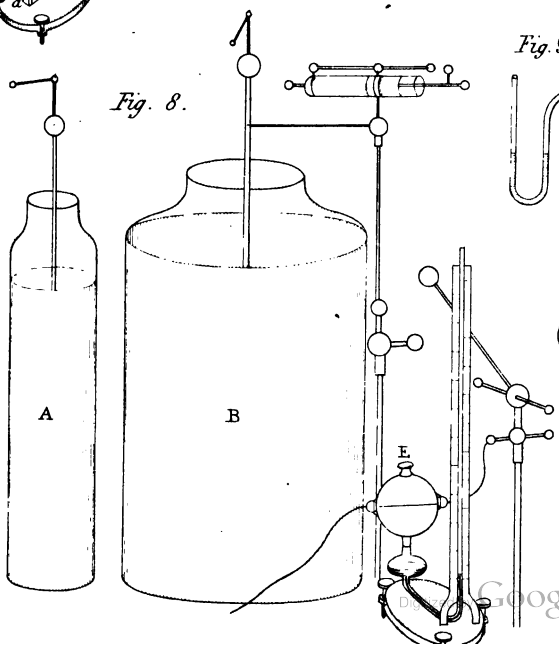

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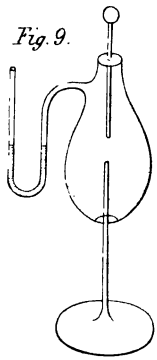

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